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BY WILLIAM NICHOLSON.

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Of the Engravings the Subjects are, 1. An improved Gun-Lock, by Mr. John Webb. 2. An Apparatus for spliting Logs of Wood, by Mr. R. Knight. 3. Mr. Eccleston's Peat-borer. 4. The Orniſcus Prægustator, as it resides in the Mouth of the Bay Alewife. 5. Outline Drawing of the Clupea Tyrannus or Bay Alewife. 6. Figure to illustrate a new Method of computing the Longitude, by Mr. Andrew. 7. Two Plates of Figures illustrative of Dr. Herschell's Memoir on the Structure of the Heavens. 8. Sir. A. N. Edelcrantz's new Statical Lamp. 9. An Ancient Magazine Gun for quick firing. 10. An Ancient Lock of thirteen hundred Combinations. 11. An Apparatus for Distillation, by Sir

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A.N. Edelcrantz. 12. A Galvanic Apparatus by Mr. Davy. 13. An Electrical Battery of Talc. 14. A Compound Artificial Magnet of four Poles. 15. The Swedish Telegraph, invented by Sir A. N. Edelcrantz. 16. An Apparatus for determining the comparative Wear of Gold, by the Hon. H. Cavendish, Esq. 17. Mr. Henry's Apparatus for measuring the Absorption of Gases. 18. A Subterraneous Graphometer, by M. Komarzewski.

Soho Square, August, 1803.

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A
JOURNAL
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MAY, 1803.

ARTICLE I.

A Drawing and Description of the Clupea Tyrannus and Oniscus Prægustator. By BENJAMIN HENRY LATROBE, *F.A.P.S.**

To Thomas P. Smith, one of the Secretaries of the American Philosophical Society.

SIR,

I BEG leave, through your means, to communicate to the American Philosophical Society, an account of an insect, whose mode of habitation, at least during some part of his life, has appeared to me one of the most singular, not to say whimsical, that can be conceived. Introductory remarks.

In the month of March 1797, illness confined me for several days, at the house of a friend on York river in Virginia, during his absence. My inability to move further than to the shore of the river, gave me leisure to examine carefully, and in more than an hundred instances, the fact I am going to mention.

Among the fish that at this early season of the year resort to the waters of York river, the alewife or oldwife, called the Description of the fish called the *bay alewife*.

* From the Transactions of the American Phil. Society, V. 77.

bay-alewife, (clupea *nondeſcripta*) arrives in very conſiderable ſhoals, and in ſome ſeaſons their number is almoſt incredible. They are fully of the ſize of a large herring, and are principally diſtinguiſhed from the herring, by a *bay* or red ſpot above the gill-fin. *(ſee the drawing)* They are, when caught from March to May, full-roed and fat, and are at leaſt as good a fiſh for the table as the herring.

Large inſect reſident in the mouth of the fiſh,

In this ſeaſon, each of theſe alewives carries in her mouth an inſect, about two inches long, hanging with its back downwards, and firmly holding itſelf by its fourteen legs to the palate. The fiſhermen call this inſect *the louſe.* It is with difficulty that it can be ſeparated, and perhaps never without injury to the jaws of the fiſh. The fiſhermen therefore conſider the inſect as eſſential to the life of the fiſh; for when it is taken out, and the fiſh is thrown again into the water, he is incapable of ſwimming, and ſoon dies. I endeavoured in numerous inſtances to preſerve both the inſect and the fiſh from injury, but was always obliged either to deſtroy the one, or to injure the other. I have ſometimes ſucceeded in taking out the inſect in a briſk and lively ſtate. As ſoon as he was ſet free from my graſp, he immediately ſcrambled nimbly back into the month of the fiſh, and reſumed his poſition. In every inſtance he was diſguſtingly corpulent, and unpleaſant to handle; and it ſeemed, that whether he have obtained his poſt by force, or by favor, whether he be a mere traveller, or a conſtant reſident, or what elſe may be his buſineſs where he is found; he certainly has a *fat* place of it, and fares ſumptuouſly every day.

thought to be eſſential to its life; and not ſeparable without deſtruction either of the fiſh, or of itſelf.

Delineation.

The drawings annexed to this account were made from the live inſect, and from the fiſh out of whoſe mouth he was taken. I had no books to refer to, then; but examining the *Syſtema Naturæ* of Linnæus, I was ſurprized to find ſo exact a deſcription of the inſect as follows (ſee *Salvii editio, Holmiæ* 1763. p. 1060. alſo Trattner's Vienna edition, ſame page).

" *Inſect. apt.* ONISCUS, PEDES XIV.

Antennæ ſetaceæ

Corpus ovale.

O. Phyſodes, abdomine ſubtus nudo, cauda ovata.

Deſcription of Linnæus under the name of *Oniſcus Phyſodes.*

Habitat in pelago; corpus præter caput, et caudam ultimam, ex ſeptem ſegmentis trunci, et quinque caudæ. Antennæ utrinque duo, breves. Caudæ folium terminale omnino ovatum; ad latera utrinque

utrinque ſubtus auctum duobus petiolis diphyllis, foliolis lanceolatis, obtuſis, cauda brevioribus. Caudæ articuli ſubtus obtecti numeroſis veſiculus longitudine caudæ."

From the particularity with which the oniſcus phyſodes is deſcribed by Linnæus, it is evident that he had the inſect before him, or a deſcription by an attentive obſerver. It appears alſo from the "*Habitat in pelago,*" that the O. Phyſodes, if this be the inſect, is found detached from his conductor. There are a few points in which the O. Phyſodes differs from my inſect. I did not obſerve the antennæ, perhaps for want of ſufficient attention, or of a microſcope. The petioli of the tail were not, to appearance, *two-leaved*, and I am certain that the ſegments of the tail, and the tail itſelf, were without the *veſiculi longitudine caudæ.*

But differing in a few points and ſaid to live *in the ocean.*

There are many circumſtances, to aſcertain which is eſſential to the natural hiſtory of this inſect. The fiſh whoſe mouth he inhabits comes, about the ſame time with the chad, into the rivers of Virginia from the ocean, and continues to travel upwards from the beginning of March, to the middle of May; as long as they are caught upon their paſſage up the river, they are found fat and full of roe. Every fiſh which I ſaw had the oniſcus in his mouth; and I was aſſured, not only by the more ignorant fiſhermen, but by a very intelligent man who came down now and then to divert himſelf with fiſhing, that, in 40 years obſervation, he had never ſeen a bay alewife without the louſe. The chad begin to return from the freſh water lean and *ſhotten,* about the end of May and beginning of June, and continue deſcending during the remaining ſummer months. No one attempts then to catch them, for they are unfit for the table. Whether the bay alewife returns with the chad, I could not learn, but it is certain that after June it is not thought worth the trouble to catch them. No one could tell me *poſitively* whether the oniſcus ſtill continues with them, but it was the opinion of my informant, that, like every other paraſite, he deſerts his protector in his reduced ſtate, for he could not *recollect* that he had ever ſeen him in the mouth of thoſe accidentally caught in the ſeine in July or Auguſt.

Whether the inſect be conſtantly in the mouth of the fiſh, &c.

I conſider, therefore, the natural hiſtory of the oniſcus, which I now communicate, as very imperfect; and it were to be wiſhed that ſome lover of natural ſcience would follow up the enquiry, by endeavouring to aſcertain whether he con-

or be the ſame as the oniſcus of Linné.

tinue with, or quit the fiſh before his return to the ocean, and alſo whether he be the oniſcus phyſodes of Linnæus, *qui habitat in pelago.*

Propoſed name.

Should he be an inſect hitherto undeſcribed, I think he might be very aptly named *oniſcus prægustator*.

The bay alewife not accurately deſcribed.

The bay alewiſe is not accurately deſcribed in any ichthyological work which I have ſeen; nor can I from my drawings, which were made with a very weak hand, venture a deſcription. From his having a regular prægustator, I would ſuggeſt that he ought to be named *clupea tyrannus.*

Leaches on the oniſcus.

The oniſcus reſembles the minion of a tyrant in other reſpects, for he is not without thoſe who *ſuck* him. Many of thoſe which I caught had two or three leaches on their bodies, adhering ſo cloſely, that their removal coſt them their heads. Moſt of the marine oniſci appear to be troubleſome to ſome one or other fiſh. The oniſcus ceti is well known as the plague of whales, and many of the reſt are mentioned in Linnæus and Gmelin, as *peſtes piſcium.*

BENJ. HENRY LATROBE, F. A. P. S.

Qu. If mentioned in Block's Hiſtory.

P. S. A gentleman well ſkilled in entomology informs me that he believes, that in Block's Hiſtory of Fiſhes, a work not to be had in Philadelphia, this oniſcus is mentioned. But, from a late examination of Gmelin and Fabricius, I am convinced that the oniſcus prægustator is a ſpecies not hitherto accurately deſcribed—Gmelin had probably ſeen the Linnæan inſect, having changed the *antennæ utrinque duo,* to *antennis quaternis,* and left out moſt of the long deſcription given by Linnæus. Neither he, Linnæus, nor Fabricius, mention the circumſtance of habitation in the mouth of the fiſh, and the induſtrious and copious Fabricius, who having changed the names of the genera, calls him cymothoa phyſodes, copies the deſcription of Gmelin, excepting the mention of the four antennæ, which in his arrangement form a character of the genus.

The

II.

The Principles on which the Purification of Fiſh-oil may be performed, and of the Uſes to which it is applicable. By ROBERT DOSSIE *.

THAT the fetid ſmell of fiſh-oil is chiefly owing to putrefaction, it is unneceſſary to ſhow; but though this be the principal cauſe, there is another likewiſe, which is, uſtion or burning the oil, occaſioned by the ſtrong heat employed for the extracting it from the blubber of the larger fiſh, and which produces a ſtrong empyreumatic ſcent that is not always to be equally removed by the ſame means as the putrid ſmell, but remains ſometimes very prevalent after that is taken away. *Fœtor of fiſh-oil is from putrefaction and empyreuma.*

In order to the perfect edulcoration of oils, there are conſequently two kinds of fœtor or ſtink to be removed, viz. the putrid, and the empyreumatic; and the ſame means do not always equally avail againſt both. *The ſame remedies do not remove both.*

The putrid ſmell of fiſh-oil is of two kinds; the rancid, which is peculiar to oils; and the common putrid ſmell, which is the general effect of the putrefaction of animal fluids, or of the vaſcular ſolids, when commixed with aqueous fluids. *The oil itſelf may become rancid; or it may putrefy as to its mixture of other matters.*

Fiſh-oil has not only rancidity, or the firſt kind of putrid ſmells peculiar to oils, but alſo the ſecond or general kinds; as the oil, for the moſt part, is commixed with the gelatinous humour common to all animals, and ſome kinds with a proportion of the bile likewiſe; and thoſe humours putrefying combine their putrid ſcent with the rancidity of the oil, and, in caſes where great heat has been uſed, with that and the empyreuma alſo. *Fiſh-oil has both theſe putrid ſmells. It contains gelatine and bile.*

The reaſon of the preſence of the gelatinous fluid in fiſh-oil is this: that the blubber, which conſiſts partly of adipoſe veſicles, and partly of the membrana celluloſa, which contains the gelatinous fluid, is, for the moſt part, kept a conſiderable time before the oil is ſeparated from it, either from the want of convenient opportunities to extract the oil, or in order to the obtaining a larger proportion; as the putrid *The gelatine is introduced from ruptured veſſels &c.*

* Communicated to the Society for the encouragement of Arts long ago, but now publiſhed by them in the original form.

effervеſcence

effervefcence which then comes on, rupturing the veficles, makes the blubber yield a greater quantity of oil than could be extracted before fuch change was produced; and the veficles of the tela cellulofa, containing the gelatinous matter, being alfo burft from the fame caufe, fuch matter being then rendered faponaceous by the putrefaction, a part of it mixes intimately with the oil, and conftitutes it a compound of the proper oleaginous parts and this heterogeneous fluid.

How the bile comes to be admixed.

The prefence of the bile in fifh-oil is occafioned by its being, in many cafes, extracted from the liver of the fifh; which is not to be fo profitably done by other means as by putrefaction; and the bile being confequently difcharged, together with the oil from the veffels of the liver containing them, combines with it, both from the original faponaceous property of bile, and from that which it acquires by putrefaction.

Train oil is particularly impure.

This holds good particularly of the cod-oil, or common train, brought from Newfoundland; which, from its high yellow colour, vifcid confiftence, and repugnance to burning well in lamps, manifefts fenfibly the prefence of bile and the gelatinous fluid; which latter, by the faponaceous power of the bile, is commixed in a greater proportion in this than in any other kind of fifh-oil.

Perfect oils are little difpofed to putrefy

A tendency to putrefy, or at moft but in an extremely flow manner, is not an abfolute property of perfect oils in a fimple or pure ftate; but it is a relative property dependant upon their accidental contract or commixture with the aqueous fluid. This is evident from the cafe of oils concreted into a febaceous form; which being perfectly oleaginous and uncombined with any water, except fuch as enters into their component parts will not putrefy unlefs water, or fomething containing it, is brought in contact with them. But the fluid animal, and moft vegetable oils, being compounded of perfect oils with other mixed fubftances, either fuboleaginous or gelatinous, have always a putrefcence *per fe*, or tendency to putrefy, without further admixture of aqueous moifture. This commixture of heterogeneous matter in fifh-oil, particularly of the gelatinous fluid and bile, gives rife to a further principle of purification than *fimple edulcoration*, or the removing the fœtor; for the prefence of fuch humours in the oil renders it fubject to a fecond putrefcence *per fe*, fuppofing the

unlefs water be prefent,

But moft oils will putrefy alone;

even after edulcoration.

the firſt corrected; makes it unfit for the purpoſe of the woollen manufacture, as the heat through which this is in ſome caſes employed, cauſes this matter to contract a moſt diſagreeable empyreuma. It alſo prevents its burning in lamps, as well from its viſcidity as from the repugnance which the preſence of water gives to all oleaginous matter. It is therefore neceſſary to free the oil from this heterogeneous matter; after which it can be ſubject only to the rancid putreſcence, or that which is proper to oils as ſuch.

Enumeration of bodies uſed for purifying oils: Acids, alkalies, oxides, neutral ſalts, vol. oil, alcohol, water, air.

The ſubſtances which have been or may be applied to the removing or preventing the effects of putreſcence, are, acids, alkalies, metallic calces, neutral ſalts, ethereal and eſſential oils, vinous ſpirits, water, and air. With reſpect to acids, though they may be applied with effect to the removal or prevention of putrefaction in mixed animal and vegetable ſubſtances, yet they have not the ſame efficacy when employed in the caſe of oils; for in a ſmall proportion, without the ſubſequent aid of alkalies, they rather increaſe than diminiſh the fœtor, and in a large proportion they coagulate the oils, and change their other properties as well as their conſiſtence. Though they might therefore be employed with the aſſiſtance of alkalies, yet requiring a more expenſive and complex proceſs, and not being moreover neceſſary, as the ſame end may be obtained by the uſe of alkalies only, they may be deemed improper for the purification of animal oils for commercial purpoſes. Alkaline ſubſtances, both ſalts and earths, are the moſt powerful inſtruments in the edulcoration of oils; but as their action on putrid oils, and the method of applying them to this end, are not the ſame in both, it is proper to conſider them diſtinctly.

Acids not eligible,

but alkalies are beſt,

Fixed alkaline carbonates increaſe the putrid proceſs;

Of alkaline ſalts it is the fixed kind only which are proper to be uſed for the edulcoration of oils. Fixed alkaline ſalts, in a diſſolved ſtate, being commixed with putrefying animal ſubſtances, appear to combine with putrid matter, and mixing with ſome of the principles, form inſtantly volatile alkaline ſalts. On the leſs putrid they ſeem to act, after their combination, by an acceleration of the putreſcent action, till they attain the degree which produces volatile ſalts. This is evident by the ſenſible putrid ferment and ſmell which appear after their commixture; but which gradually abating, the oil is rendered ſweeter, much lighter-coloured and thinner.

Their

and are very useful in purifying the oil;

Their great ufe in the edulcoration of fifh-oil arifes, therefore, from their converting fuch parts of the gelatinous fluid and bile as are highly putrefied, inftantly into volatile falts, and caufing a rapid putrefaction of the other parts; by which means the oil is freed from them by their diffipation. They do not, however, equally act on the parts of the oil on which the empyreumatic fcent depends, unlefs by the affiftance of heat; for when they are commixed with the oils without heat, in proportion as the putrid fmell diminifhes, that becomes more fenfibly prevalent. The ultimate action of lixiviate falts on animal oils, except with refpect to the empyreuma, feems to be the fame either with or without the medium of heat; for the fame urinous and putrid fmell, gradual diminution of the colour, and fetid fcent, happens in one cafe as in the other, except with regard to the acceleration of the changes; and fuch falts, where the purification is required to be made in a great degree, are a neceffary means, as they are more effectual than any other fubftance that can be employed.

but they do not remove empyreuma.

The alkaline carbonates render part of the oil folid;

The ufe of lixiviate falts alone is not, however, the moft expedient method that can be purfued for the edulcoration of oils, for feveral reafons. If they be ufed alone, cold, in the requifite proportions, they coagulate a confiderable part of the oil, which will not again feparate from them under a very great length of time; and when they have deftroyed the putrid fcent, a ftrong bitter empyreumatic fmell remains. The fame inconvenience, with relation to the coagulation of part of the oil, refults when they are ufed alone with heat. The fuper-addition of common falt, (which refolves the coagulum and counteracts the faponaceous power of the lixiviate falt, by which the oil and water are made to combine) is therefore neceffary; and the expence arifing from the larger proportion of lixiviate falt, requires it to be employed if no other alkali be taken in aid, and renders the junction of alkaline earths with it extremely proper in the edulcoration of oils for commercial ufes. Lime has alfo an edulcorative power on animal oils; but it has alfo fo ftrong a coagulative action, that the addition of a large proportion of alkaline falts becomes, when it is ufed, neceffary to reduce the concreted oil to a fluid ftate; and therefore this fubftance alone is not proper for that purpofe. The combination of lixiviate falt with

but this is corrected by common falt.

Alkaline earths.

Lime is good but it coagulates.

as do alfo foap lees.

with lime, or the solution commonly called soap-lye, has an effectual edulcorative action on fetid oils; but it makes a troublesome coagulation of part of the oil, if no common salt be employed, and must be used in such large proportion, if no alkaline earth be added, as renders the method too expensive.

Lime alone is improper;

Lime has a power of combining with and absorbing the putrid parts of the gelatinous fluid and bile, when commixed with oil; and effects, either with or without heat, a considerable edulcoration of fetid oils; but it combines so strongly with them, either cold or hot, that the separation is difficult to be effected, even with the addition of brine; and the oil, when a large proportion of it is used, can scarcely be at all brought from its concreted to a fluid state, but by an equivalent large proportion of lixiviate salt: the use of lime therefore, alone, is improper, or even in a great proportion with other ingredients. But when only a lesser degree of edulcoration is required, a moderate quantity, conjoined with an equal or greater weight of chalk, which assists its separation from the oil, may, on account of its great cheapness, be employed very advantageously: it will in this case admit of precipitation from the oil by the addition of brine. It may be also expediently used, when lixiviate salt is employed with heat for the most perfect purification of oils; for it will in that case give room for the diminishing of the quantity of lixiviate salt, though the proportion be nevertheless so restrained as not to exceed what the proportion of lixiviate salt (just requisite for the edulcoration) can separate from the oil.

unless in small proportion and with chalk and brine.

Chalk less effectual; but may be used with other matters.

Chalk has an absorbing power similar to lime, but in a less degree on the putrid substance of oil; it does not, however, combine so strongly with the oil as to resist separation in the same manner, and is therefore very proper to be conjoined either with lixiviate salts or lime, as it renders a less quantity of either sufficient, and indeed contributes to the separation of the oil from them.

Magnesia and alumine rejected.

Magnesia alba or the alkaline earth which is the basis of the sal catharticus, and the singular earth which is the basis of alum, both have an edulcorating power on fetid oils; but, like lime, have too strong an attraction with them to be separated so as to admit of the reduction of the oil from the concreted state to which they reduce it; and therefore, as they are not superior in efficacy to lime and chalk, but much dearer

or more difficult to be obtained, they may be rejected from the number of ingredients that are proper for the purifying of oils, with a view to commercial advantages.

Sea-salt useful in mixtures but not alone.

Sea-salt has an antiseptic power on the mixed solid parts of animals; but used alone, or dissolved in water, it does not appear to lessen the putrid fœtor of oils, but on the contrary rather increases it. If, after their commixture with it, they are subjected to heat, it rather depraves than improves the oils; but though by its own immediate action on them it conduces so little to the edulcoration of oils, yet it is a medium for the separation of water and the alkaline substances requisite to be employed to that end. It is of great utility in the edulcorative processes: for when alkaline salts or earths combine with the water necessary to their action on the oils, or themselves form coagulums or corrections with it, a solution of salt will loosen the bond and dissolve the close union; so that the oil being separated will float on the aqueous fluid, while the earth, if any be in the mixture, will be precipitated and sink close together to the bottom of the containing vessel.

Muriate of potash, sulphate of soda or of potash, &c. of little value.

Sal catharticus, glauber salt, nitrum vitriolatum, tartar, and other neutral salts, though they counteract putrefaction in the mixed or solid parts of animals, seem to have little effect on oils with respect to their edulcoration, and cannot therefore be ranked amongst the substances proper to be used for that purpose.

Oxide of lead is very powerful,

Lead reduced to the state of a calx, either in the form of minium or litharge, has a strong edulcorative power on fetid oils, and is indeed applied to that end, with respect to one kind of vegetable oil, for a very bad purpose, considering its malignant qualities on the human body.

In the case of train-oil, which will scarcely ever be considered among the esculent kinds in this country, the same objection against its use would not lie; and employed either with or without heat, it is a powerful absorbent both of the putrid and empyreumatic parts that occasion the fœtor.

but was rejected from prejudice.

As however there may be some prejudice against its use, even in any way, and as it is not absolutely necessary, I have not given it a place among the ingredients of the processes I recommend.

The

The ochrous earth of iron, commonly called red ochre, has an abforbing power on the putrid parts of oil, but combines fo ftrongly, that the feparation is tedious even with the addition of brine; if, neverthelefs, it is added when chalk and lime have been fome time commixed with the oil, as in procefs the firft, it will promote the edulcorative intention, and will fubfide along with them; and, as it has fome advantage without increafing the expence, unlefs in the moft inconfiderable degree, its ufe may be expediently admitted in that procefs.

Red ochre may be ufed in mixture.

Effential and ethereal oils are applicable to the prevention of putrefaction in the mixed and folid parts of vegetables, but are not fo to the edulcoration of fetid oils; and if they had the defired effect, they would not, on account of their price, anfwer the commercial end, unlefs the due effect was produced by adding them to the oils in a very fmall quantity.

Volatile oils inefficient and coftly.

The fame holds good of fpirits of wine as of effential and ethereal oils, both with refpect to their efficacy and the expence.

So likewife are alcohol and ether.

Water has an edulcorative action on fetid oils, by carrying off the moft putrid parts of the gelatinous fluid or bile, in which, as was above explained, the principal fœtor refides, if the quantity added be large, and an intimate commixture be made of them by ftirring them together for a confiderable time: this only partially removing thofe heterogeneous putrefcent fubftances, the remaining part foon acquires the fame ftate and the oil again grows fetid, though not to the fame degree as before.

Water by agitation improves oils, but they foon putrefy again.

Water is, however, a neceffary medium for the action of falts and the feparation of alkaline earths and calces of metals, when they are employed for the edulcoration of oils, as will appear from a confideration of my proceffes.

It is neceffary in the compound proceffes.

Air edulcorates oil by carrying off the moft putrid parts, which are neceffarily extremely volatile. It may be made to act on them either by fimple expofure of them to it with a large extent of furface, or by forcing it through them by means of ventilators as has been practifed by fome dealers; but is now, I believe, neglected on account of their finding the improvement of oils by it not adequate to the trouble, as the gelatinous matter and bile, not reduced to a certain degree of putrefaction, being left behind, putrefy again to nearly the fame degree as before.

Air is ftill lefs effectual.

It

Reſumption. Lime, chalk, ſea-ſalt, and alkali, are the materials for purifying oils.

It appears from theſe ſeveral obſervations, that the cheapeſt ingredients which can be uſed for the edulcoration of train-oils, are lime and chalk, which may, with the addition of a proper quantity of ſolution of ſea-ſalt or brine, be made to procure a ſeparation of them from the oils, according to proceſs the firſt, ſo as to anſwer for ſome purpoſes; that the lixiviate ſalt is the moſt powerful purifier of oils, and, with the aſſiſtance of chalk and brine, will, without heat, according to proceſs the ſecond, effect a very conſiderable degree of edulcoration; and that lixiviate ſalt uſed with heat, with the addition of lime and chalk, to ſave a part of the quantity which would otherwiſe be neceſſary, and of brine to procure a quick ſeparation, will perform an edulcoration ſufficient for all commercial purpoſes, according to proceſs the third; but that calcined lead and the ochrous earth of iron may, perhaps, be applied in ſome caſes with advantage, where the oil is not deſigned for eſculent uſe.

PROCESS THE FIRST.

For purifying Fiſh-oil in a moderate Degree, and at a very little Expence.

Proceſs 1. Moderate purification. Chalk and ſlacked lime agitated with oil; ſalt water added; repoſe; decantation.

TAKE an ounce of chalk in powder, and half an ounce of lime ſlacked by expoſure to the air; put them into a gallon of ſtinking oil, and, having mixed them well together by ſtirring, add half a pint of water, and mix that alſo with them by the ſame means. When they have ſtood an hour or two, repeat the ſtirring, and continue the ſame treatment at convenient intervals for two or three days; after which, ſuperadd a pint and a half of water, in which an ounce of ſalt is diſſolved, and mix them as the other ingredients, repeating the ſtirring, as before, for a day or two. Let the whole then ſtand at reſt, and the water will ſink below the oil, and the chalk ſubſide in it to the bottom of the veſſel. The oil will become clear, be of a lighter colour, and have conſiderably leſs ſmell; but will not be purified in a manner equal to what is effected by the other proceſſes below given; though, as this is done with the expence of only one ounce of ſalt, it may be practiſed advantageouſly for many purpoſes, eſpecially as a preparation for the next method, the operation of which will be thereby facilitated.

PROCESS THE SECOND.

To purify, to a great Degree, Fiſh-oil without Heat.

TAKE a gallon of crude ſtinking oil, or rather ſuch as has been prepared as above mentioned, and add to it an ounce of powdered chalk; ſtir them well together ſeveral times, as in the preceding proceſs; and after they have been mixed ſome hours or a whole day, add an ounce of pearl-aſhes, diſſolved in four ounces of water, and repeat the ſtirring as before. After they have been ſo treated for ſome hours, put in a pint of water, in which two ounces of ſalt are diſſolved, and proceed as before: the oil and brine will ſeparate on ſtanding ſome days, and the oil will be greatly improved both in ſmell and colour. Where a greater purity is required, the quantity of pearl-aſhes muſt be increaſed, and the time before the addition of the ſalt and water prolonged.

Proceſs 2. Greater purification without heat. Agitate with chalk: add ſolution of pearl-aſhes: ſalt water: repoſe: decantation.

If the ſame operation is repeated ſeveral times, diminiſhing each time the quantity of ingredients one half, the oil may be brought to a very light colour, and rendered equally ſweet in ſmell with the common ſpermaceti oil.

Repetition of the proceſs.

By this proceſs the cod-oil may be made to burn; and, when it is ſo putrid as not to be fit for any uſe, either alone or mixed, it may be ſo corrected by the firſt part of the proceſs, as to be equal to that commonly ſold: but where this proceſs is practiſed in the caſe of ſuch putrid oil, uſe half an ounce of chalk and half an ounce of lime.

PROCESS THE THIRD.

To purify Fiſh-oil with the Aſſiſtance of Heat, where the greateſt Purity is required, and particularly for the Woollen Manufacture.

TAKE a gallon of crude ſtinking oil, and mix it with a quarter of an ounce of powdered chalk, a quarter of an ounce of lime, ſlacked in the air, and half a pint of water; ſtir them together; and when they have ſtood ſome hours, add a pint of water and two ounces of pearl-aſhes, and place them over a fire that will juſt keep them ſimmering, till the oil appears of a light amber colour, and has loſt all ſmell except a hot, greaſy, ſoap-like ſcent. Then ſuperadd half a pint of water in

Proceſs 3. Greateſt purification. Agitate with chalk and lime: add water and pearl-aſh, and ſimmer it over a fire: add ſalt and water and boil: repoſe: decantation.

in which an ounce of ſalt has been diſſolved; and having boiled them half an hour, pour them into a proper veſſel, and let them ſtand till the ſeparation of the oil, water, and lime be made, as in the preceding proceſs. Where this operation is performed to prepare oil for the woollen manufacture, the ſalt may be omitted; but the ſeparation of the lime from the oil will be ſlower, and a longer boiling will be neceſſary.

If the oil be required yet more pure, treat it, after it is ſeparated from the water, &c. according to the ſecond proceſs, with an ounce of chalk, a quarter of an ounce of pearl-aſhes, and half an ounce of ſalt.

Obſervations on Proceſs the Firſt.

Obſervations.

This proceſs may be performed on any kind of fiſh or ſea-oil that is putrid and ſtinking, and will improve it in ſmell, and generally render the colour lighter, if previouſly dark and brown: it will alſo conduce to render theſe oils fitter for burning, which are, in their crude ſtate, faulty in that point; but it will not meliorate them to the full degree they admit of even without heat, and ſhould therefore be practiſed when only a moderate improvement is required.

The dregs may be ſaved.

Secondly, When the oil is taken off from the dregs and brine, the dregs which ſwim on the brine ſhould be taken off it alſo, and put into another veſſel of a deep form; and on ſtanding, particularly if freſh water be added and ſtirred with them, nearly the whole remaining part of the oil will ſeparate from the foulneſs; or, to ſave this trouble, the dregs, when taken off, may be put to any future quantity of oil that is to be edulcorated by this method, which will anſwer the ſame purpoſe.

Obſervations on Proceſs the Third.

Proceſs 3 is beſt for train-oil.

Firſt.—This is moſt advantageouſly performed on train-oil, called vicious whale-oil; and the more putrid and foul it may be, the greater will be the proportional improvement, eſpecially if there be no mixture of the other kinds of fiſh-oils, particularly the ſeal, which do not admit of being edulcorated perfectly by means of heat, but require other methods: but when the vicious oil is pure from admixture of others, however ſtinking it may be, the bad ſmell will be removed by this proceſs duly executed, and the brown colour changed to a very

very light amber; and these qualities will be much more permanent in this than in any crude oil, as it will not, from the degree of purity to which it is brought, be subject to putrefy again under a great length of time, whether it be kept open or in close vessels.

The oil in this state will burn away without leaving the least remains of foulness in the lamp: and, being rendered more fluid than before, will go further, when used in the woollen manufacture, than any other kind, and will be much more easily scoured from the wool. It will then burn clear, &c.

If nevertheless there be any branches of the woollen manufacture which require the use of a more thick and unctuous oil, this may be rendered so by the addition of a proper quantity of tallow or fat, of which a certain proportion will perfectly incorporate with the oil, the fluidity and transparency being still preserved, as well as all the other qualities that render it suitable to the intended purpose. This may be most beneficially done by adding a proper quantity of the refuse grease of families, commonly called *kitchen-stuff*, which being put to the oil, when moderately heated, will immediately dissolve in it, and let fall also its impurities or foulness to the bottom of the vessel, and render the purified admixture a considerable saving to the manufacturers. Kitchen-stuff may be added when a thick oil is wanted.

Secondly. The different qualities and dispositions of different parcels of vicious oil with respect to edulcoration, render various proportions necessary of the ingredients to be used. The quantities stated in the above process are the least which will effect the end in general, and frequently greater will be required; but this may always be first tried; and if it be found, after six or eight hours simmering of the mixture, that no gradual improvement is making in the smell and colour, but that the oil continues the same in those particulars, and remains also mixed with the chalk and lime, and in a thick turbid state, a fourth or third part of the first quantity of pearl-ashes should be added, and the simmering continued till the oil be perfect. As the quantity of the water is lessened by the evaporation, it is necessary to make fresh additions from time to time, that there may be always nearly the original proportion. The quantities of ingredients may be varied,

Thirdly. If it be inconvenient to give the whole time of boiling at once, the fire may be suffered to go out and be rekindled at any distance of time; and if, in such case, a small and the boiling performed at different times.

proportion

proportion of pearl-aſhes diſſolved in water be added, and the mixture ſeveral times ſtirred betwixt the times of boiling, it will facilitate the operation. The time of boiling may be alſo much ſhortened, if the chalk, lime, and pearl-aſhes, be added for ſome days before, and the mixture freqently ſtirred.

PROCESS THE FOURTH.

Which may be practiſed alone, inſtead of Proceſs the Firſt, as it will edulcorate and purify Fiſh-oil to a conſiderable Degree, ſo as to anſwer moſt Purpoſes, and for Proceſs the Third, when the whole is performed.

Proceſs 4. Purification by lime water.

TAKE a gallon of crude ſtinking oil, and put to it a pint of water poured off from two ounces of lime ſlacked in the air; let them ſtand together, and ſtir them up ſeveral times for the firſt twenty-four hours; then let them ſtand a day, and the lime-water will ſink below the oil, which muſt be carefully ſeparated from them. Take this oil, if not ſufficiently purified for your purpoſe, and treat it as directed in Proceſs the Third, diminiſhing the quantity of pearl-aſhes to one ounce, and omitting the lime and chalk.

ROBERT DOSSIE.*

III

An Eſſay on the Fecula of Green Plants. By Profeſſor PROUST.

(Concluded from Page 278. Vol. IV.)

Fecula not precipitable by water.

I BELIEVE Parmentier was the firſt who doubted, and it was not unreaſonable in his time, whether the tincture of fecula in alcohol was reſinous, becauſe it was not precipitated by water. But when we conſider that water can never detach it from the gluten, that alcohol, oils and greaſe have excluſively that property, and laſtly, that this ſubſtance, when ſeparated from the alcohol and left to itſelf, is a fat body, te-

* The dregs, remaining after the ſundry proceſſes above mentioned, will form an excellent manure; as has been ſince noticed in Dr. Hunter's Georgical Eſſays.

nacious

nacious and infoluble in water; it muft be acknowledged that there is no other product in vegetables to which it approaches fo nearly. But we fhall find, that, in order to give it this character more perfectly, it is only neceffary to add to it a little oxigen.

But rendered more completely refinous by oxigenation.

The oxigenated muriatic acid, in a few days, bleaches and hardens the green refin; it then becomes ropy, like boiled turpentine and its colour is very diffufible in water; but if the green part of the fecula belonged to the coloured juices, contained in dying ingredients, oxigen would not convert it into a refin. At prefent, when experience has taught us not to fix fuch ftrict limits to vegetable products as heretofore, becaufe we fo often find them united by intermediate qualities, we fhall not be furprifed to fee that a refin, at its maximum of divifibility, can mix with water. Do we not find that camphor, effential oils, both animal and vegetable, farcocolle, &c. are completely diffolved by water? Yet we do not from that circumftance exclude fuch products from the claffification which analyfis affigns to them.

When it turns brown.

The green fecula acquire, by the action of oxigenated muriatic acid, that tawny brown colour which indicates the decay of the leaves in winter, and their aqueous diffufion becomes abundantly turbid. On the whole, then, we may infer, that though the colouring matter of the fecula cannot refift the action of water, when extracted by alcohol, it is neverthelefs, in all its other qualities, a fubftance truly refinous; and though this product, which is one of the moft curious of the vegetable kingdom, becaufe it adorns it by its various combinations, has not been admitted in the *fyftem of chemical knowledge*, yet the chemifts who have been the moft occupied with it, fuch as Rouelle, Danel, Sage, Parmentier, &c. have not thought it lefs worthy of their inveftigations.

This refin quits potafh, to unite in preference with animal matter like a dye.

This refin, when diffolved in potafh, quits it to unite with filk, which it tinges of a clear green, but too fugitive to become ufeful; notwithftanding which it refifts the action of verjuice; but its attraction for gluten in preference to the vegetable fibre, is conformable to eftablifhed principles; for, in general, colouring bodies attach themfelves to animalifed fubftances, rather than to the fibres of flax, hemp, or cotton. There is then fomething in the fecula analogous to wool, to filk, &c. It is the gluten.

V. Let us now examine the fecula by tefts more adapted to difclofe the new characters of animalifation.

The fecula putrefies like an animal body.

If, in fummer, the fecula, either boiled or raw, be kept under water, in lefs than twenty-four hours it yields a difagreeable fmell, and foon exhales an excrementous fetor which continually increafes, and, to which it might be dangerous to be long expofed. The infectious miafma arifing from it, inftantly difcolours metallic writings; and its liquor, which may be compared to a cadaverous fanies, alfo rapidly blackens plates of filver.

Steeping of flax.

It is, without doubt, from the corruption of this principle, rather than from any other caufe, that the pernicious exhalations from hemp and flax, while fteeping, arife. Running waters, which are equally proper to feparate the flax as ftanding ones, quickly carry off their extractive juices; there can only remain the fecula intangled in the green fibres which is fufceptible of being deftroyed by fteeping.

The liquor, which at the end of a year, remains above the rotten fecula, contains fulphurated hidrogen, carbonate of ammonia, and gluten diffolved by the intervention of the latter principle.

The liquid very obftinately ftercoral.

This liquor has alfo this peculiarity, that it preferves its ftercoral fmell after long boiling. The product of its diftillation contains carbonate of ammonia, combined with a principle of infection, which does not blacken metallic folutions, and with the nature of which I am not at all acquainted.—Acids, by precipitating the fecula and faturating the ammonia, do not weaken it; this has, for a long time, induced me to think, that though the effluvia from a mafs of animal putrefaction may ferve as a vehicle to the phofphorus and fulphur contained in it, yet thefe combuftibles are not alone, the caufe of the infection. For there is a great difference, for example, between the fmell of rotten fifh or flefh, of corrupted fecula, of ftinking cheefe, and thofe of phofphorated and fulphurated hidrogen.

The infection peculiar in putrefying bodies.

On Putrefaction.

What, then, is putrefaction? A change of which we have very few clear ideas.

Putrefaction is not deftruction,

When fecula, curd, flefh, or any organic matters in general, have paffed through a certain period of this change, which we are

are accuſtomed to call putrefaction, all at once they ſtop in a ſtate of permanence, in which, unknown combinations ſeem to fix, and as it were to ſalt them, to embalm them, ſo as to inſure their duration in this new ſtate and to ſecure them from all ſubſequent deſtruction.

When, for example, the curd, the fecula, the gluten, or the fleſh, after having paſſed through all the changes of an infection, which is often deadly, and thoſe derangements of colour and of form which disfigure them, have at laſt arrived, the one to a cheeſy ſtate; the vegetables, the dung-hills to mould, turf, or poudrette;—fleſh itſelf has not been annihilated after an ichorous ſtagnation of fifteen years: all ſtop at this point, without being able to paſs the limit, without ever attaining, at leaſt under our eyes, that final diſſolution which muſt terminate their exiſtence, or reduce them to a mere earthy matter, inert *non ab ſimilem cineribus*, to uſe the expreſſion of Stahl, in ſhort, to a ſtate in which no trace of the radical of their organization can be diſcovered. but a permanent ſtate.

Putrefaction of this kind in ſtrictneſs takes place no where. No ſooner do we perceive the derangement of the organization of an animal or vegetable matter, lividity, infection, but we conclude that this proceſs has commenced; and without conſideration, we confound theſe appearances, which belong to a ſpecies of fermentation, little known, with the effects of that which alone is the true putrefaction, if its end agrees with the notions we form of it; if it be truly an operation determined by nature to analyſe and reſolve into their laſt principles, thoſe ſubſtances which are ſubmitted to it. Complete deſtructive putrefaction is no where ſeen.

Hence we may conclude, that abſolute putrefaction is a thing we are totally unacquainted with. But let us return to the fecula; it is time to attend to that which is in ſuch a ſtate of diviſion as to paſs the filtre.

VII. We will take as an example, the filtered juice of cabbage, one of thoſe plants which yields it moſt abundantly. And, at the ſame time, the better to demonſtrate the difference between this ſecond fecula and albumen, we will ſubmit the latter to the ſame proofs. The white of an egg beat up with a pound of water, and filtered, will furniſh the teſt liquor we want. Fecula from cabbage juice; compared with albumen.

1ſt. Into water heated to 50 degrees, immerſe two matraſſes, one with the filtered juice, the other with albumen. In an It ſeparates from water by leſs heat.

 inſtant

inſtant the juice will be clouded with cheeſe-like flocks which fall to the bottom; but at this temperature, the albumen does not experience the ſlighteſt change.

Fecula, though greatly diluted falls by heat.

2d. Place on a furnace two matraſſes, the one with juice mixed with twenty parts of water, and the other with albumen. The fecula, however diluted it may be, will nevertheleſs be entirely ſeparated by the heat; thus clearly proving its inſolubility. With reſpect to the water of the albumen, as it heats, it opaliſes, without loſing its tranſparency; it boils, and becomes contracted, but does not depoſit flocks, or any thing reſembling fecula. And, if at laſt the evaporation be compleated in an open veſſel, it ends by leaving only a coat of white egg. Darcet has already ſhewed that albumen diſperſed in a great quantity of water, is no longer ſeparable by heat. Albumen is a ſoluble mucilage, fecula is not; and the temperature which coagulates the latter produces no change on the ſtate of the firſt.

Fecula continually ſeparates.

3d. The water of albumen remains ſeveral days unaltered; the juice of plants, on the contrary, is in a continual ſtate of change which never ceaſes to diſturb its tranſparency. Filter the juice, it becomes turbid; repeat the filtering, it again becomes clouded; in ſhort it continually depoſits white fecula.

Albumen acts on teſts like an alkali: fecula not.

4th. Albumen changes the juice of violets green, and reſtores reddened turnſole to blue.

The white fecula, waſhed, does not produce either of theſe changes. And how ſhould it? The juices of cabbage, hemlock and many others redden turnſole. Beſides the property of changing it to green is not poſſeſſed by the albumen itſelf: it is known to be owing to a mixture of alcali.

They differ in habitude with alcohol;

5th. Alcohol ſeparates light, tranſparent, glary flocks from the water of albumen, which retain, on the filtre, the appearance of boiled white of egg. The juice of plants only yields to alcohol a whitiſh opake powder, which quickly falls to the bottom of the veſſel.

with acids;

6th. All the acids, hidro-ſulphurated water, and ammonia precipitate the fecula diſſolved in the juices; but theſe re-agents operate no change on the water of albumen.

with ox. mur. acid;

The oxigenated muriatic acid precipitates and oxidates the white fecula; the ſame acid firſt oxidates and then precipitates the albumen.

7th, Cryſtallized

7th. Cryſtallized carbonate of potaſh, magneſia, ſea-ſalt, muriate of potaſh, ſal-ammonia, ſaltpetre, &c. thrown into a filtered juice, cauſe the fecula, which by its nature is but little ſoluble, to precipitate in proportion as they diſſolve. The water of the albumen is not diſturbed by any of theſe ſalts. and with compound ſalts.

Conſequences.

The white fecula depoſited ſpontaneouſly, or by alcohol, acids, ſalts, &c. is inſoluble in water. The albumen is the reverſe: the acids which precipitate the fecula, do not alter the ſolution of albumen. Fecula inſoluble in water; albumen ſoluble.

No ſalt is capable of depriving the albumen of water; but with the fecula it is different; its affinity for water is ſo weak that there is none which does not deſtroy it; and, conſequently precipitate it.

The white of egg dried and afterwards ſoftened is reſtored, in bulk, in opacity, in whiteneſs to boiled albumen. Not ſo the white fecula; it becomes of a deep brown. Nay the greateſt number are entirely blackened in drying, ſuch as thoſe of cabbage, creſſes, *ſolanum lico perſicum*, &c. and if they are ſoftened in water, they will never aſſume the appearances which are known in white of egg. In ſhort, this fecula is nothing but a part of the gluten which forms the baſis of the green fecula. If, for example, the fecula of white cabbage, ſeparated by the filtre, be compared with that which is obtained from its juice by heat, and both be deprived of their colouring parts, the ſmalleſt difference will not be diſcovered. But, above all, the white fecula is the moſt eaſily diſſolved, becauſe it is not, like that which is green, in a ſtate of combination which oppoſes it. All plants contain a portion of gluten, which, not having been vivified by the light, remains without colour. Other ſtriking differences. Green and white fecula.

The cabbage, the endive, the (eſcarolle) and the plants blanched by the art of the gardener, alſo yield white fecula, but in much leſs quantity than when they are permitted to remain green. The ſtalk of the cabbage and that of hemlock afford pale fecula in compariſon with that obtained from their leaves. But in general it is not neceſſary that vegetables ſhould ſhow much colour outwardly to indicate their poſſeſſing much gluten.

Separation by heat is not an exclusive character of albumen.

gluten. The small houseleek, as we shall see presently, yields an abundant fecula of a deep colour, and particularly rich in wax.

VIII. But it will be said, that the albumen being the only product in which the property of coagulating by heat has been noticed, it would appear natural to conclude that, &c. But the milk of almonds also separates by heat, alcohol, acids, &c. This is a fact which has been always known in pharmacy, and nevertheless a conclusion has never been formed from this slender appearance that emulsions contained white of egg, because even if the characters of animalisation had been perceived in the principles of the emulsions, they ought likewise to have exhibited other striking marks of resemblance before they could have been considered as albuminous *.

The characters of fecula and gluten are not the same throughout, &c.

It is also in this point of view that the gluten of fecula must be regarded, because it is neither tenacious, nor elastic, nor fermentable, like that of cheese †. In announcing it to chemists as a product analogous to that from wheat, Rouelle only brought forward part of the characters which constitute the agreement, those alone which belong to the nature of the component parts, because the external marks of resemblance do not exist; for the same reason, my object, in defending the labours of this great master, is much more to retain in the catalogue of his discoveries that of an animalised matter found more immediately in the leaves, than of gluten properly so called, because this in fact, is the discovery which the author of the "System" has rendered doubtful in his work. Break

* Rouelle had a much better foundation than he was aware of, for comparing the green juice of plants to an emulsion. The cheese separated from the milk of almonds, by some one of these means, being washed and dried, yields an oil by expression, and afterwards all the products of caseum, by distillation. This, without doubt, is the reason why almonds and all sorts of nuts, afford so great a quantity of nitrogen, with nitric acid.

The thin milk of almonds contains gum, a little extractive matter, and sugar, which is either that of the cane, or that which I discovered in the grape, and which I shall describe when treating of fermentation.

† The gluten of wheat is susceptible of a fermentation which is peculiar to it. The gases disengaged are carbonic acid and tolerably pure hidrogen in abundance. I shall hereafter resume this subject.

the

the aggregation of animalifed matters, take away the forms of filk, horn, wool, feather, &c. and it is clear that then being confidered only as to their conftitnent parts, they would be albumen, gluten, fibre, and all that can be ftated, becaufe if the conftituent parts are, in all refpects, the fame, (which even now has not hitherto been examined, and which neverthelefs is the only proper means of diftinguifhing them from each other,) nothing would remain but to afcertain the proportions in which nature has combined them, in order to give them their refpective properties or being.

But, it may be added, that if the albumen be not feparated from the juices with characters as diftinct as may be wifhed, this muft be afcribed to the extract, the falts, and the acids which are conftantly united with it, and which cannot but difguife it a little; it is particularly in the wafhings of flour that it muft be fought, to be obtained in fuch a ftate of purity as to put its nature paft all doubt. Let us fee then what the wafhings of flour will afford. Whether fecula be albumen.

IX. The water of flour like a recently filtered juice, is in a ftate of change continually increafing, and which does not ceafe until the acid arifing from the fermentation of the faccharine principle has completed the precipitation of the gluten. Water of flour refembles a filtered juice.

All the acids, all the falts which have been applied to the juices operate in a fimilar manner upon the wafhings of flour; alcohol does the fame, but not vinegar, becaufe it diffolves the gluten. In a word, it is not by coagulating the gluten that acids feparate it from juices and the water of meal, becaufe ammonia and falts do the fame, but rather by attracting the folvent of a fubftance which appears to borrow its folubility from a pure and fimple divifion, but not from an affinity fimilar to that which unites gums, fugar, or albumen to water. The gluten feparated by acids,

The water from flour expofed to a heat of fifty degrees, parts with its gluten like the juice of plants. Neither can folubility be imparted to the gluten by diluting it with a large quantity of water. It is precipitated by the flighteft impreffion of heat. and by moderate heat.

I have collected to the quantity of an ounce of gluten, feparated by heat from fuch wafhings, to be kept in its own moifture; it fermented and produced vinegar and ammonia. At prefent, after two years, it is a dark mafs, cellular, odorous and favoury, like the cheefe from gluten.

To

Albumen has not been discovered in vegetables.

To conclude; albumen has not yet been discovered in vegetables. Though it is not to be thence inferred, that it is impossible it should be formed there as well as in animals. The time in which we live, more fertile than ever in observations, convinces us daily that there are few products, either of one kingdom or the other, which can be truly considered as exclusive. Nevertheless it must be admitted that in endeavouring to establish the existence of albumen to the exclusion of the gluten of green plants, the learned author of the "System" has depended too much on the feeble support of a simple appearance. It appears to me that, before stating the existence of the albumen, he ought to have strengthened his first opinion with facts more conclusive than the single one of concrescibility. Let us not however overlook that, in so vast an enterprise as his, it is very difficult for an author to bring all his materials together with equal precision.

Remarks on other objects.

It will be in the same disposition that I shall extend these conclusions to the other products, placed by Fourcroy, without sufficient examination, among the glutinous substances of vegetables.

There exists, says he, an observation more exact and more positive than that of Rouelle upon the presence of this glutinous matter in the vegetable texture which forms linen, paper, &c. p. 296. vol. vii. It is sufficient to recall this passage to the recollection of its author. More details on my part, would have too much the appearance of censure. I think Fourcroy will suppress it in a new edition as well as that relative to the paste of mallows. If this paste be entitled to be placed in the class of the animalised products of vegetables, almond paste, and those of frangepane, eggs, marmalade, &c. are equally deserving to be so arranged.

Bird-lime is a kind of turpentine.

With respect to the bird-lime mentioned in the same chapter, it is universally known to be only a kind of turpentine, an inflammable aromatic resin, soluble in alcohol, formed by vegetation in the fibrous texture of the holly-oak, in the fruit of the elder, perhaps in its bark, and in other plants, but in no respect a glutinous substance.

Green fecula soluble in potash and leaves woody matter.

X. Potash easily dissolves green fecula, and divides it into two parts; one unites itself to the solvent and the other is separated in the form of a green powder which is not affected by a new quantity of potash. This powder, being washed and dried,

dried, yields by diſtillation the products of white wood, of linen, that is to ſay, nothing ammoniacal. It is the woody part which uſually accompanies the fecula in conſequence of the trituration.

This ſolution has all the characters of an animal ſolution; it exhales ammonia, an odour which is that of a woad vat; it blackens the ſilver evaporating baſon, and emits, by the action of acids, an effluvium which darkens the writing of white metals. The ſolution has animal characters.

But in this caſe, as in the ſoap of wool, great part of the fecula experiences a loſs which deranges the nature and proportion of its radicals. Acids only ſeparate a ſmall quantity of the fecula. The remainder acquires an extractive character which diſpoſes it to unite with water. Neither alcohol nor acids can ſeparate this new extract from ſalts. It is of a fawn colour, and muriate of tin precipitates from it a duſky lake. But the fecula cannot be recovered by acids.

With reſpect to the other, when collected and waſhed on the filtre, it is worthy of notice that it has not loſt the property of hardening by the heat of boiling water. Properties of the ſeparated matter.

Alcohol acquires a deeper green from the precipitated fecula than from that which is freſh. This ariſes from the reſin, which not being deſtructible like the gluten, is united in greater quantity to that which eſcapes the deſtruction. As to its other properties, this fecula, by diſtillation, yields ammoniacal products.

XI. An acid of 18 or 20 degrees diſengages azote abundantly from green fecula; a ſtronger acid diſſolves them with facility, and ſeparates a little powder which is the ligneous fragments of the plant. Acids ſeparate azote from fecula, &c.

However ſparingly the nitric acid is uſed, it is but ſeldom that cryſtalliſed oxalic acid can be obtained. It is reſolved into carbonic acid and water.

The ſolutions of fecula always contain the yellow bitter principle of Welter, ſulphuric acid, benzoic acid, oxalate of lime and fat. If the ſolution of a fecula containing iron, ſuch as the ſolanum lycoperſicum, be precipitated with acetite of lead, a powder compoſed of oxalate and phoſphate of lead, and oxide of iron is obtained. On heating it with the blowpipe it burns, even the lead is diſſipated and nothing remains but a globule of phoſphate of iron.

When

When a vegetable product contains nitrogen, fulphur, phofphorus, benzoic acid, fat, yellow bitter, iron in abundance, no doubt can remain of its belonging to the clafs of animalifed fubftances.

Of Wax.

Wax is a vegetable matter.

XII. Wax is the work of vegetation, and not of the bees. It is, I think, in deriving nourifhment from the gluten which accompanies it in the duft of the ftamina that they make the feparation. This duft yields ammonia in abundance, which has led me to believe that it contains gluten; and now that I have difcovered wax in certain fecula, I am of opinion, that if this duft was treated with nitric acid, wax would be obtained.

It may be had from fecula.

The fecula of the fmall houfeleek yielded a quantity that furprifed me. This wax is white, dry, brittle and without fmell; it cannot be confounded with the febaceous products obtained from other fecula, fuch as thofe of hemlock and folanum. Meffrs. Fernandez and Chabaneau examined it to fatisfy themfelves; they chewed it, and were convinced that this product was a perfect wax.

The fecula of green cabbage alfo yielded it, but in much lefs quantity. The wax appears to me to be the varnifh fpread over the plants by vegetation, doubtlefs to preferve them from the effects of wet, which might injure them. It is this varnifh which divides the rain and the dew into the filvery pearls upon the leaves of cabbages, poppies and fo many other plants which afford this agreeable fpectacle in our gardens. It is alfo the wax which the curious gardener who gathers a plum, a fig, or a grape is careful not to rub off with his fingers.

It covers plants;

At Paris, an orange on removing the paper in which it has been wrapped from its leaving Portugal, will be found covered with a farinaceous coating, which may be taken off with the blade of a knife, and then brought to a candle and melted to afcertain its nature.

The fecula of opium alfo contains a fat which from its firm confiftence, nearly refembles wax, and which has been known to many opiologifts.

and filk.

Finally, raw filk is alfo covered with a coating of wax which is carried off, with the colour, by alcohol, and is feparated from that fluid by cooling.

Of

Of ſome other fecula leſs known.

When five or ſix pounds of ſaffron are operated upon at the ſame time in order to obtain the volatile oil and extract, a fine powder is obſervable in the decoction, which renders it cloudy, precipitates, and may be ſeparated by ſtraining. This powder waſhed aſſumes in drying the contraction and horn-like appearance of the green fecula in ſummer; it putrefies rapidly and becomes food for worms if care be not taken. In the fire this fecula yields all the products of gluten. With alcalis and lemon-juice, it tinges ſilk of very brilliant yellow. Fecua from Saffron.

Borage.

A plant may contain gluten in two ſtates: the one, in the fecula; the other, diſſolved in its juice by means of potaſh. Such is the juice of borage; clarified, it is thick and of a light blue colour; a few drops of acid ſeparate from it a cheeſy curd which may be collected on the filtre, and is nothing but gluten. from borage;

Elder. Dwarf Elder.

The berries, in the midſt of an highly coloured, very gummy, and ſlight by ſaccharine juice, contain a fecula as green as that of ſpinach when it has been well purified from its red colour. Alcohol extracts from it a green tincture; the reſidue is a gluten not at all different from that of fecula. from elder and dwarf elder.

On cruſhing theſe berries their bird-lime ſticks to the fingers; it is of the ſame conſiſtence as that from the holly-oak: ſubmitted to fermentation theſe juices yield a very ſmall quantity of ſpirit of a diſagreeable ſmell. It is ſucceeded by an aſtoniſhing quantity of very good diſtilled vinegar.

Buckthorn.

Its juice, which contains a very nauſeous bitter extract with gum and a little ſugar, is thickened with a greeniſh mud, which is ſeparated from it by heat and fermentation. This pulp, well waſhed, is of a clear green; it is gluten mixed with a ſmall quantity of fibre. It affords carbonate of ammonia, &c. from Buckthorn:

The Roſe.

Its petals yield by trituration a fine lightly coloured fecula, which affords the ſame products as gluten. from the roſe;

Grapes.

from grapes. Fecula is found abundantly in them, it is that which makes the ley of wines; but to ſpeak of this product would be to anticipate what I have to ſay on fermentation. Laſtly, gluten is found in quinces, apples and, without doubt, in other fruits; it is found in the acorn, the horſe-cheſnut, the Spaniſh cheſnut, rice, barley, rye, peaſe, and beans of all ſorts. I ſhall return to this ſubject in treating of the difference between wheat which has germinated and that which has not undergone this operation.

IV.

Deſcription of a Borer for draining Boggy Land. *By* THOMAS ECCLESTON, Esq. *

Chief difficulty of draining bogs. The newly cut drain riſes up.

THE greateſt obſtacle to the effectual draining of many boggy lands, conſiſts in the earth in the bottoms of the ditches or drains when newly cut, and more eſpecially if made to any conſiderable depth, riſing from the preſſure of the waters contained in the bog, by which the new-cut drains and ditches are frequently ſo nearly filled up, as to impede the flowing of the water they were intended to carry off, and thereby rendering the work comparatively ineffectual.

Remedy by boring.

There are different layers, or ſtrata, in moſs or peat lands, which will not allow the water eaſily to filtrate through them, yet are of ſo ſoft and ſpongy a nature as to riſe from the preſſure of the water contained in the bog.

It becomes neceſſary to give a free vent to the above confined water, effectually to drain ſuch lands. This has been moſt ſucceſsfully done by the Augre, a model of which I have herewith preſented to the ſociety.

Method inſufficient.

A common augre, or even a pole, will force a paſſage, and give vent to the water for a ſhort time; but owing to the peat being only preſſed ſideways, and not cut out, the parts ſoon join again, and the paſſage of the water of courſe becomes completely obſtructed; but by means of this augre, a cylindrical column of peat of ſix inches diameter will be clearly cut out and taken thence, and a free paſſage maintained for a very conſiderable ſpace of time.

* Soc. of Arts. 1801.

The

The firſt experiment made, produced a clear augre-hole of the above dimenſions, four yards in depth, in one hour; and the water, which had been pent up in the bog, roſe above the level of the bottom of the ditch, from four to ſix inches; and the bottom of the ditch, which was previouſly very ſoft, and had begun to ſwell and riſe, in a few days became more firm and ſolid, and this in ſo great a degree, that when cleared it remained without ſwelling or riſing in the leaſt. It will conſiderably reduce the expence of draining ſuch lands, by rendering them ſo firm as to cauſe the firſt end-drains to ſtand. but the large augre cuts a durable hole.

The moſt proper depth to bore, depends on the ſituation. Where the moſs lands lie low, and are in danger of being flooded, no greater depth than what is abſolutely neceſſary for draining the ſurface ſhould be bored, as, by deep boring, the land may be ſunk ſo low as to be liable to inundation.

A. Plate II. Fig. 2. The cutter of the borer, which penetrates the peat. B. The body of the borer, ſix inches in diameter. C. The aperture through which the peat introduced by boring is drawn out. D. A portion of the iron bar of the borer, to the upper part of which a croſs handle is to be fixed. Deſcription.

V.

Deſcription of an Improvement in the Gun-Lock by which the caſual Diſcharge of the Piece is prevented. By Mr. JOHN WEBB *.

I HAVE taken the liberty of ſending to the ſociety an invention of mine, to prevent the accidents which frequently attend the uſe of fire-arms, and which may be applied to the gun-locks now in common uſe. It is contrived on ſuch a principle, that when it is on full cock, and the trigger pulled in the common manner, it returns to the half cock only, unleſs, at the ſame time that the trigger is pulled, the preſſure of the thumb is applied on a ſpring placed upon the butt or ſtock of the gun; in which caſe it gives fire in the uſual manner. The inventors letter.

* Tranſactions of the Society of Arts, 1802. To the Secretary; and the Society gave the inventor a bounty of twenty guineas.

manner. The intent of this invention is to guard againſt the caſualties which ariſe when fire-arms are left loaded, or the misfortunes which frequently happen from twigs of trees or buſhes catching the trigger when ſportſmen are paſſing through hedges.

I hope it will meet the approbation and encouragement of the Society, and am,

S I R,

Your humble Servant,

JOHN WEBB.

Deſcription of Mr. JOHN WEBB's *Gun-Lock, Plate I. Fig.* 1, 2, 3, 4, 5. *The letters of the ſeveral Figures correſpond together in the general Deſcription.*

Deſcription of the lock.

A is the cock—B, the hammer—C, the main ſpring—D, the tumbler—E, the large ſear—F, the ſmall ſear—G the ſear ſpring—H, the ſhank or arm of the large ſear—I, the ſhank or arm of the ſmall ſear—K, the thumb-piece—L, the trigger—M, the lever of the thumb-piece—N, the ſpring which holds the thumb-piece up, when not preſſed upon by the thumb.

Fig. 1, is an interior view of the lock at full cock.

Fig. 2.—The ſame lock at half cock.

Fig. 3.—The lock when down.

Fig. 4.—The lock fixed in the gun-ſtock, in order to ſhow the thumb-piece K and the trigger L, with their mode of action. When the gun is held cocked in the uſual manner, ready to fire, and the trigger L is pulled by the finger, the thumb, being preſſed at the ſame time on the piece K, raiſes, by means of the lever M, moveable on a pin in its centre, the ſhank I of the ſmall ſear, and admits the cock to give fire as in the common way; whereas, if only the trigger L is pulled, the lock ſtops at the half cock I; further motion being prevented by a notch in the ſmall ſear. A ſpring, N, ſcrewed to the ſtock, returns the thumb-piece to its place, when the thumb is taken off.

Fig. 5 ſhows, on a larger ſcale, the conſtruction of the tumbler, large and ſmall ſears, the ſear-ſpring, and the manner in which they riſe out of the bents of the tumbler.

VI. *Deſcription*

VI.

Defcription and Account of a fimple Apparatus for breaking-up Logs of Wood by the Explofion of Gunpowder. By Mr. RICHARD KNIGHT *.

SIR,

I HAVE frequently obferved the great difficulty, labour, and lofs of time experienced in breaking-up logs of wood, particularly for the purpofe of fuel; fuch as the ftumps and roots of large trees, which remain after the felling of timber, many of which, efpecially fuch as confift of the harder and more knotty kind, as oaks, elms, yews, &c, are frequently left to rot in the ground, in order to avoid the neceffary expence of breaking them to pieces in the common way, which is generally effected by the axe, and driving a fucceffion of iron wedges with a fledge hammer; a laborious and tedious procefs. Sometimes gunpowder is ufed, by fetting a blaft in a fimilar way to that in mines or ftone-quarries. This method, though lefs laborious than the former, is tedious, is attended with feveral difficulties, and requires confiderable experience and dexterity, or the plug will be more frequently blown out than the block rent by the explofion. With a view, therefore, to obviate thefe difficulties, I have conftructed an inftrument, a fketch and defcription of which I now inclofe for your approbation. The fimplicity of its conftruction and application is fuch as almoft to preclude an idea of its originality; but as it has hitherto appeared entirely new to all my acquaintance, and as I do not know that any thing of the kind has ever before been prefented to the public, I am induced to think it may not be unacceptable; and fhould it appear to you an object worthy the attention of the Society of Arts, I fhall be happy in making it public through a channel fo highly refpectable; and will, immediately on being favoured with your opinion, tranfmit to the fociety a complete inftrument with the neceffary appendages, and a more minute defcription of its mode of application.

Remarks on the object of Mr. Knight's apparatus.

I am, SIR, Your obedient humble Servant,

RICHARD KNIGHT.

Fofter-lane, March 16, 1802.

* Tranfactions of the Society of Arts, 1802. The Inventor received the Silver Medal.

The following Articles were ſent to the Society. See Plate II. Fig. 1 and 2.

A, the rending or blowing ſcrew, with a wire B, for the purpoſe of occaſionally clearing the touch-hole, previous to the introduction of the quick-match.

C, an auger proper to bore holes, to receive the charge of the ſcrew.

D, a gouge, to make an entrance for the auger.

E, a lever, to wind the ſcrew into the wood, with a leather thong F attached to it, in order to faſten it occaſionally to the ſcrew, to prevent its being loſt, in caſe it ſhould be thrown out when the block is burſt open; a circumſtance which does not often occur: for in all my experiments, when the wood has been tolerably ſound, I have always found the ſcrew left fixed in one ſide of the divided maſs.

A roll of twine is to be ſteeped in a ſolution of nitre, for the purpoſe of a quick match, or train, to diſcharge the powder, by thruſting a piece thereof down the touch-hole, after taking out the wire B.

Account of the inſtrument actually uſed.

The firſt that was made was for J. Lloyd, Eſq. of St. Aſaph, the late member for Flint, who, having a great quantity of timber on his eſtate, conſiders it a conſiderable acquiſition; and at Overton-Hall, laſt ſummer, ſpoke ſo favourably of it, in my preſence, to Sir Joſeph Banks, that he immediately ſent for his ſmith, and requeſted I would give him the neceſſary inſtructions for making one; but as I left that part of Derbyſhire ſoon after, I had not an opportunity of ſeeing it finiſhed. Since my return home, I have had ſeveral made, ſimilar to that which I now preſent to the Society, which are better finiſhed, and have ſharper threads than ſmiths in general have an opportunity of giving them.

Letter from Mr. Lloyd on the ſame ſubject.

DEAR SIR,

Account of the uſe and application of the blaſting ſcrew.

AFTER you left us laſt autumn, at Sir Joſeph Banks's, his ſmith, who is a remarkably good workman, beſtowed much needleſs time and trouble in making a blaſting-ſcrew; for he finiſhed it in the higheſt ſtyle of poliſh, and, I think, made the thread of the worm too fine, or at leaſt finer than was needful. However, it anſwered moſt completely, and very much to Sir Joſeph's ſatisfaction, who lamented he had not ſeen ſuch a contrivance

contrivance many years ago, when a relation of his ufed to amufe himfelf with fplitting the roots of trees, &c. in the common way. I have ufed the *blafting-fcrew*, for fo I fhall call it, all the laft and preceding winter, with the greateft fuccefs, and have gained many loads of fuel, which otherwife would have been fuffered to rot, as the expenfe and labour in clearing the roots in the ordinary way renders the fuel fo procured too expenfive; and fince I have had the fcrew, I have obferved fome hundreds of roots in a rotting ftate in other places, from the want of knowing that there was fuch a contrivance as the fcrew. I think you would ferve the public in no fmall degree, by divifing fome method of making its ufe known to the world.

When I was at Overton, fome pieces of very tough, knotty, clofe-grained oak were picked from the timber-heap, for the ufe of the Gregory lead-mine, by Sir Jofeph Banks's direction, and the fcrew fevered fome pieces four or five feet in length, and nine or ten in diameter, throwing them fome feet afunder, to the furprife of the miners, who were affembled on the mine-bank. Sir Jofeph took the fcrew with him to Revefby-Abbey, in Lincolnfhire, where, I underftand, he had fome large roots, that had lain by many years as ufelefs; and I dare fay he will give you a good account, and bear teftimony to the utility of the invention. We have ufed it without a fingle accident; but my neighbour, Lord Kirkwall, having procured one to be made by that which I had from you, one of his fervants, in his Lordfhip's abfence, I prefume, put too much powder into the hole, and the fcrew was blown as high as a one-pair-of-ftairs window, and paffed through it into an apartment where a perfon then happened to be, but without any farther mifchief than the lofs of a pane of glafs. Any one who ufes the inftrument will foon learn what depth of fcrew will be fufficient to fplit any root in proportion to its ftrength, taking care that the fcrew has fufficient hold to refift the force of the gunpowder, before the root is cleft. I think much powder may be faved by ufing a cotton match, impregnated by a folution of faltpetre, or any of the combuftible matters generally made ufe of in fire-works; and by the ufe of the cotton the hole through the fcrew may be leffened, which will add to the action of the confined powder, though a ftraw filled with powder, in the manner in which the miners ufe it, anfwers very well. Should any one be timid in ufing the fcrew, a chain or rope may eafily be attached to the fcrew, and that fixed to any log, or faftened to a ftake driven into

into the ground. If wood is rotten, the ſcrew cannot act. I aſſure you, that when I go abroad, I conſtantly ſee great quantities of roots in a rotten ſtate, about almoſt every farm-houſe, which would not be the caſe if the utility of the inſtrument were made public. I am,

Your much-obliged friend, and humble ſervant,

Wygfair, March 26, 1802. J. LLOYD.

VII.

Letter from Mr. DALTON, *containing Obſervations concerning the Determination of the Zero of Heat, the thermometrical Gradation, and the Law by which denſe or non-elaſtic Fluids expand by Heat.*

Mancheſter, April 20th, 1803.

To Mr. NICHOLSON.

SIR,

Remarks on a letter in our laſt number, concerning the real zero of heat.

IN your laſt Number, page 221, a correſpondent has adverted to an hypotheſis which I ſuggeſted in my account of experiments on the expanſion of elaſtic fluids. It was, that "the repulſive force of each particle [of gas] is exactly proportional to the whole quantity of heat combined with it," when ſubject to a given preſſure. Having previouſly found that 1000 parts of any elaſtic fluid, at temperature 55°, were expanded to 1325 at 212°, it followed from the hypotheſis, that the quantity of heat in air, of 55°, is to the quantity in air of 212°, as $\sqrt[3]{1000} : \sqrt[3]{1325}$, or as 10 : 10.9834 : from which the real zero is deduced at 1573° below the freezing point of water. I had moreover noted, that for $78\frac{1}{2}$° (as the abovementioned writer has juſtly corrected it) the expanſion of air was 167 parts, and for the other $78\frac{1}{2}$°, only 158.—From theſe data, calculating, as he ſays, *upon my hypotheſis*, he has deduced two other points for the real zero; namely, 1714°, and 1486°: theſe inconſiſtent reſults, he thinks, tend to diſcredit, either the hypotheſis, or the accuracy of the experiments. Now he muſt give me leave to obſerve, that the two laſt deductions are *not* derived from my hypotheſis, but from another totally inconſiſtent with it; namely that the mercurial thermometer is an accurate meaſure of heat. For, upon the hypotheſis that the real temperature is as the cube root

root of the aërial ſpace, it will be found, that when the mercurial thermometer ſtands at $133\frac{1}{2}°$, a proper equidifferential thermometer would ſtand at 139° 3. Hence no inconſiſtent reſults can be derived from the above data.

The mercurial thermometer is not a correct meaſure of heat.

Before the merits of the hypotheſis in queſtion can be decided, we muſt have an inſtrument to meaſure equal increments of heat. The mercurial thermometer equally divided is not that meaſure. De Luc has ſhewn that the true mean heat betwent freezing and boiling, is ſeveral degrees below 122° per ſcale. Even Dr. Crawford, who has manifeſted more deſire than any other perſon to find the mercurial thermometer an accurate meaſure of heat, has concluded from a variety of experiments, all of which prove that mercury expands more in the higher than in the lower part of the ſcale, that the thermometer is *nearly* accurate only. Now it has been ſhewn, that elaſtic fluids obſerve a general law of expanſion, whether by heat or diminiſhed preſſure. Why ſhould not liquids do the ſame?—I expect to be able in a ſhort time to demonſtrate that water and mercury agree in one and the ſame law or rate of expanſion from the two points at which they freeze and boil reſpectively: and yet theſe two liquids are apparently the moſt diſcordant in this reſpect. It would certainly be a moſt extraordinary anomaly, and inconſiſtent with the ſimplicity of the other laws of nature, if mercury alone ſhould be excepted from a common law by which all other liquids expand, not uniformly, but increaſingly, with equal increments of heat. The truth is, I believe, that the ſame quantity of heat which raiſes the mercurial thermoter 2° at the freezing point (of water), will raiſe it about 3° at the boiling point. A mercurial thermometer graduated according to the hypotheſis I have ſuggeſted, that the increments of heat are as the increments of the cube roots of the manometrical ſpaces, (in which caſe it no where differs more than 7° from the common equal graduation for 180) does, I apprehend, in all caſes, indicate the true mean temperature, when hot and cold waters are mixed together; at leaſt, I have not been able as yet to find any deviation.

Expectation that all liquids will be found to obſerve the ſame law of expanſion.

Seguin's determination of zero.

With reſpect to the diſcordance, exhibited by Seguin, in the reſults of experiments to determine the real zero, it may ariſe from an error in the theory, or from errors in the experiments

riments. I am inclined to afcribe it to the latter. The fimplicity of the doctrine of heat, maintained by Drs. Irvine and Crawford, is to me fo ftrikingly apparent, when contrafted with the now prevailing, but, I think, unfounded diftinctions of *combined* and *uncombined*, or free caloric, that I fhould be very unwilling to difcard it; but upon clear proof of its being incompatible with the phenomena.

I am your obliged friend,

J. DALTON.

VIII.

Memoir containing the Phyfical and Chemical Examination of the Teeth. By CIT. JOSSES, *of Rennes* *.

The bones long ago analyfed;

AMONG thofe animal matters which have been analyfed with the moft attention, the bones may be felected. For a long time, they were believed to be formed of earthy fubftances, the particles of which were connected by a peculiar *gluten:* at prefent it is afcertained, that they are a true faline concretion, known by the name of phofphate of lime, mixed with a certain quantity of gelatine. The experiments made to prove this have been fo often repeated, particularly in the extraction of phofphorus, and are fo well known, that they cannot now be called in queftion.

but not the enamel of the teeth.

But though there be no longer any doubt as to the offeous fubftance, the fame cannot be faid of the covering which cloaths the furface of the expofed part of the teeth. This covering, conftantly confounded in the general clafs of bones, feems never to have undergone a thorough examination, alone, by an accurate decompofition. All the authors who have treated of this fubject, appearing to have done it too inattentively, have committed errors, which it is effential to correct. Though it has been fuppofed, that this covering was formed of materials analogous to the offeous part, to which it adheres, it is neverthelefs true, that it differs from it effentially, in its phyfical and chemical qualities, and that it poffeffes peculiar properties, as may be eafily fhewn.

This laft is the author's object.

The intention of this work is, therefore, to afcertain the compofition of this covering, to fhew its phyfical and che-

* Annales de Chemie, xliii. 2.

mical

mical characters, and to point out the error of confidering it as a common offeous body, having no other difference but the denfity arifing from the compactnefs of its conftituent parts.

I fhall firft concifely point out the phyfical characters of the enamel of the teeth, and then proceed to the chemical inveftigation. This analyfis muft more fully elucidate the phyfiological inferences I fhall then offer, to prove that nature has affigned particular functions to this fubftance, which differs abfolutely from the bones.

Phyfical characters of the enamel of the teeth.

When examined on the furface of the teeth, the enamel is white, fmooth, polifhed, tranfparent, very brittle, and of extreme hardnefs: its fracture fhews a very diftinct regular chryftallization, formed of an affemblage of fmall brilliant cryftals, very compact, and inclining to the fhape of needles. On all the furfaces which it covers, as well as in the interior of certain teeth where it is found, it is difpofed in radii, a little oblique and horizontal, almoft perpendicular to the body of the bone, forming at the point of contact with it two angles, the upper one re-entering and acute, the other inferior, re-entering and obtufe. This fubftance is found in this manner, in all animated beings which have teeth.

It appears chryftallized in its fracture.

From this fhort fketch of the phyfical characters of the teeth, it will be feen, that it is impoffible to avoid concluding, that the enamel is very different from the offeous body to which it adheres; for which reafon I have often propofed to myfelf, to make a fcrupulous examination of it. A favourable circumftance offered itfelf, and furnifhed me with an opportunity of making the experiments I had long intended: an opportunity which I fhould not eafily have found any where but at Paris. Before recounting the means I have employed, I ought to fay that thefe experiments have been performed in the laboratory of the medical fchool.

Its characters differ from thof of the bony teeth.

The analogy of the work I wifhed to undertake, with the interefting refearches on animal fubftances, which continually occupy the profeffors of chemiftry of the medical fchool: the flattering approbation and eagernefs expreffed by profeffor *Deyeux*, in affifting me with his advice; the zeal manifefted by the pupils of this laboratory, to whom the care of performing the operations is confided: in fhort, a multitude of facilities left me no doubt that my hopes would be crowned with fuccefs.

One

One of the firft things to be attended to, was to procure the enamel pure: this operation was not without difficulty as will be eafily conceived, from the clofe adherence which this fubftance preferves with the cffeous body, from the time the germ of the tooth is developed aod has paffed through the regular periods of the formation of the enamel, and of offification. Neverthelefs, a fufficient quantity may be difengaged from the bony part, by the affiftance of chemical agents, which, without producing any effect on the animal, act very readily on the bone; fuch are the means which will be pointed out, and which I have employed to perform the experiments I am about to defcribe.

Having procured fome human teeth, as well as thofe of feveral animals, I expofed them to the action of various chemical agents.

The enamel afforded a very flight indication of gelatine.

I then filed off the enamelled furface of fome teeth, without reaching the offeous part, I caufed the filings refulting from this operation to be boiled in water, and I fubmitted the liquor to the action of tanin. I obtained fo flight a precipitate, that this operation fcarcely fhowed the prefence of gelatine. On the contrary, the offeous body, in the fame proportions, and by the fame procefs, yielded an abundant precipitate.

Papin's digefter foftens the bone, but not the enamel of teeth.

I expofed whole teeth in water, to a degree of heat fuperior to that of ebulition, by means of Papin's digefter; the enamel preferved its hardnefs and its figure, but the bony part was foftened and became friable.

The enamel does not burn like bone.

In a naked fire, the enamel does not burn like a bone; the fmell arifing from the igneous decompofition of an animal fubftance, is fcarcely perceptible; it is not converted into charcoal, but is flightly browned and decrepitates.

Diftillation extricates fcarcely any thing from it.

When carefully feparated from the body of the bone, and diftilled in a retort, this fubftance fhews only a faint trace of animal product; it does not afford, like the bone, a great quantity of phlegm, of oil, of corbonate of ammoniac, or of carbonate hydrogen gas; and in all its phyfical and chemical characters, it prefents differences which keep it diftinct from the offeous body.

It is foluble in acids.

However unfufceptible of change the enamel may appear to be, it is neverthelefs foluble in all the acids, but with remarkable differences, which depend on their radicals, their

 concentration,

concentration, their combined or insensible caloric, and on that caloric which being only interposed between their constituent particles, is the cause of their various temperature. Hence arise varieties in the affinities, which change the attractions, and produce different variations in the results, of which I shall have occasion to speak.

Nitric acid acts rapidly on the enamel, and slowly on the bone.

Having plunged human teeth and those of several animals into nitric acid, I observed that it acted rapidly on the enamel, and slowly on the bone. The first was soon entirely dissolved, and I could only perceive the osseous part, which in its turn, but in a much longer time, disappeared in an excess of acid. When the solution was compleated, I tasted it; it appeared to me still strongly acid, and I found that its taste differed from that of nitric acid. To judge of the nature of this liquor, I filtered it, and submitted it to some experiments, which showed that it contained phosphoric acid, disengaged, without doubt, by the nitric acid.

The nitric solution affords phosphoric acid.

Muriatic acid.

Teeth digested with muriatic acid, gave similar results to the former.

Sulphuric acid acts less on the enamel, and more on the bone.

But the same effects were not produced when I employed sulphuric acid: whether concentrated or diluted with water, hot or cold, the effervescence with the enamel, which I noticed in the other acids, did not take place with this. It appeared at first to refuse to dissolve this substance, but I soon saw it act on the bony part. In a little time I observed it precipitate a whitish salt, which I knew to be a true sulphate of lime. This solution also preserved its acid taste, of which I could not deprive it, notwithstanding the pains I took by adding fresh teeth. This acidity was owing to the phosphoric acid which had been disengaged.

The enamel separated by sulphuric acid.

The experiments which I have just described with the three acids, did not satisfy me; they had been performed on teeth furnished with their enamel. The results were therefore confused, and it was necessary to examine them in a direct manner. I resolved, therefore, to operate upon the enamel perfectly freed from the osseous part: I endeavoured to separate it with a file, but this method appeared too laborious and too tedious to furnish me with a sufficient quantity. I had recourse to the sulphuric acid: the preference I gave to it, was pointed out by the last experiment I have noticed above, which had left me considerable portions of the enamel undissolved,

undiffolved, and perfectly deprived of the offeous fubftance. This method fucceeded beyond my hopes, by weakening the acid a little, which I made warm to haften the effect. Thus I foon procured detached enamel in fufficient quantity, amply to fupply the experiments I propofed to make.

Its appearance. The enamel, feparated in the manner I have defcribed, was wafhed in feveral waters before it was ufed, by which means it was freed from the fulphate of lime, attached to its furface; it was then dried. Examined then with a magnifying glafs, it fhowed tranfparent parts, the fracture of which, when broken, prefented very well-defined needle-fhaped cryftals. This enamel was very hard, and perfectly refembled the fragments which I had broken in fplinters, from the upper part of a tooth, by ftriking it with a hammer. It only differed from it in having a whiter appearance fuperficially, owing to the lime having been made more evident. Some pieces of this enamel were laid on burning coals; they decrepitated, and their particles were thrown to a great diftance.

Ignition left a white powder; Other fragments, put into a red-hot crucible, having produced the fame effect, I pulverized a certain quantity, and heated it for fome minutes, in another crucible. This powder firft loft its very white colour, and became a little grey; but by continuing the fire, it regained its whitenefs: it then crumbled eafily between the fingers; its tafte was alcaline; when mixed with water, it was partly diffolved, and the folution refembled lime-water.

which was lime. Another part of this powder triturated with muriate of ammonia, foon produced the decompofition of this falt, and difcovered the ammonia; and laftly, the calcined enamel was equally foluble in nitric, muriatic, and fulphuric acids, without any apparent difference. The folutions, examined by different re-agents, correfponded with folutions of lime. Though it was evident, from thefe experiments, that lime was the bafis of the enamel, yet it remained to be difcovered what it was united to, before calcination.

Diftillation of this enamel drove off no fluid. To obtain proofs on this head, I weighed four grammes of enamel, prepared, as I have defcribed, with fulphuric acid; and, after having wafhed them well with a brufh, to remove the fmall quantity of fulphate of lime, which was found during the feparation of the offeous part, and precipitated on its furface, I pulverized them, and proceeded to diftillation in a coated

coated glafs retort. The fire was urged fo as to bring the retort to a red-heat, and after more than an hour, I remarked in the neck of it, a white fublimation, very light. This was the only product I obtained, and notwithftanding my care in keeping up the fire, no fluid was condenfed.

When I judged that the operation was at an end, the apparatus was difengaged, and I haftened to examine the fublimate formed in the neck of the retort. The quantity was fo fmall, that I had much trouble in collecting it; it was diffolved by nitric acid. I then had reafon to believe that it was lime, volatized by the action of the fire; neverthelefs, as I had been ftruck with a flight ammoniacal odour, I introduced into the neck of the retort, a match moiftened with muriatic acid, and I obferved that fome white vapours were quickly formed. A fmall portion of white fublimate, containing fome ammonia.

The refidue of the diftillation was a whitifh powder, whofe tafte was flightly cauftic: it was diffolved by water, and more readily by acids, and changed tincture of violets to green. Its folution, treated with different re-agents, yielded a true lime. Refidue of lime.

On reflecting upon the products obtained during the operation, I have given an account of, I thought I obferved a kind of analogy with thofe afforded by oxalate of lime. To fatisfy myfelf how far this opinion might be well founded, I determined to proceed in the analyfis of the enamel, according to the method pointed out by *Fourcroy* and *Vauquelin*, in the interefting work publifhed by them on urinary calculi, in which they fhew that the fpecies of concretion called mural ftone *(pierre murale)*, is entirely formed of oxalate of lime and an animal fubftance. Whether the enamel be oxalate of lime.

I therefore pulverized fome enamel, and boiled a certain quantity in liquid carbonate of potafh. The liquor was fcarcely boiling, when I perceived a flightly penetrating odour, nearly refembling that of ammonia. I then introduced into the neck of the matrafs, a paper match, dipped in concentrated nitric acid; immediately an abundant vapour was formed, fuch as is always feen when ammoniacal gas is brought into contact with that difengaged from nitric acid. Enamel diffolved in potafh. Ammoniacal vapour.

When the ammoniacal odour had ceafed, I withdrew the matrafs, and placed it on a fand-bath. After twenty-four hours digeftion, I found at the bottom of the matrafs, a white precipitate, above which floated a limpid liquor; it was decanted

canted and filtered, and then poured upon a new quantity of powdered enamel, and I proceeded as before: at the end of a fecond digeftion of twenty-four hours, its tafte did not appear to be any longer alcaline.

Examination of the alkaline folution, in order to detect oxalic acid, without fuccefs.

Following the directions of the fame authors, I examined the carbonate of potafh in which I had digefted the enamel, with acetite of lead and barytes; and obtained a very abundant white precipitate. Thefe refults, fimilar to thofe obtained by the above two chemifts, from mural or oxalite calculi, or calculi of oxalate, gave reafon to fuppofe that the oxalic acid being combined with the lime in the enamel of the teeth, formed there, alfo, an oxalate of lime. To afcertain this, I tried to decompofe the two precipitates I have mentioned, but all my endeavours on this point were without fuccefs *.

Enamel feparated by the digefter.

Fearing that the fulphuric acid, which I had ufed in my preparatory proceffes, had altered fome of the component parts of the enamel of the teeth, I procured fome by another method, and recommenced my operations. Papin's digefter juftified my hopes; it afforded me an eafy method of obtaining an abundant quantity, completely difengaged from the offeous part, and which had not experienced any change from an acid. The enamel obtained by this new method, and fubmitted to the fame experiments, no longer yielded fimilar refults.

* If my fufpicions had been realifed, it will be perceived, that the oxalate of lime being thus difcovered completely formed by nature in living animals, and neceffary in the organifation of the teeth, the oxalic acid would become a ftriking part of the beautiful theory of the two learned men whom I have cited. This would have afforded a natural explanation of the calculous oxalates in the urinary concretions, as well as of the oxalic acid fometimes obferved in humours, though their phenomena are always thought to be unufual, and to arife from morbific affections.

As the formation of the enamel takes place in the firft years of life, and neceffarily ceafes after the laft dentition, it may eafily be conceived, that the reflux of the oxalic acid, or the liquid oxalate lime, by way of urine, which has fo many points of refemblance to offeous fubftances, muft have given rife to the concretion of the mural calculi, which are more common in youth than in age, as *Fourcroy* has well obferved in his works. But after much labour, I found myfelf compelled to abandon my firft expectations, and to endeavour, by new inveftigations, to difcover the truth.

This

This fubftance diftilled in a retort, did not now emit any ammoniacal odour, nor did it produce any fluid. The refidue of the diftillation was of a grey colour, which became white by calcination, and by the intenfe heat of the fire it acquired the hardnefs of porcelain; it no longer had the alcaline tafte, it no longer decompofed muriate of ammonia; it no longer changed fyrup of violets green, water no longer acted on it; it fhort, the enamel not diftilled and fubmitted to thefe latter experiments, afforded the fame refults as the refidue of the diftillation: I again placed fome in digeftion with diluted fulphuric acid, for the purpofe of treating it like the offeous fubftances from which phofphoric acid is to be feparated, and I obtained fimilar refults; that is to fay, acid phofphate of lime in folution in the liquor, with fulphate of lime, and fragments of the enamel not decompofed. Thefe are the falts whofe prefence has been afcertained from examinations by the ufual methods.

The refidue of this when diftilled was hard like porcelain and refembled the original enamel, &c.

It feemed to be phofphat of lime.

From the great difference of thefe refults obtained in both methods of treating the enamel of the teeth, it will be feen that at firft I was led into an error by the effect of the fulphuric acid, which, in the preliminary preparation left confiderable portions of enamel, whofe phyfical characters caufed me to believe them unchanged.

The enamel feparated by fulph. acid is not pure.

I have thought it ufeful for the intereft of fcience, to make known thefe furprifing differences, they may be advantageoufly applied in the medical art, and affift in explaining the phenomena, as I had occafion to point out elfewhere.

IX.

A New Method of Clearing obferved Lunar Diftances of the Effects of Parallax and Refraction, for the Purpofe of determining the Longitude at Sea or Land. By Mr. J. ANDREW. *Communicated by the Author.*

LET A, B, C, D, Pl. 3. Fig. 3. be four given points on any fphere, and fo conftituted that they fhall all lie on the fame imaginary plane; and let the fphere be fuppofed to be cut fo by this plane, that the lines AB, BC, CD, DA, and BD, joining the given points, may be all of them right lines, or chords of great circles which may be fuppofed to have paffed through

Principle of the method demonftrated.

through thoſe points before the cutting of the ſphere. Farther, let the chord AB be parallel to the chord DC, and ſince the four points A, B, C, D, lie in the ſection of a ſphere, that is, in the circumference of the ſame circle, the chord AD will be equal to the chord BC, and the diagonal BD to the diagonal AC. For the ſame reaſon (becauſe ABCD is a quadrilateral figure inſcribed in a circle) the rectangle under the diagonals BD and AC is equal to the ſum of the rectangles under the oppoſite ſides AD, BC, and AB, DC; or, becauſe BD and AC are equal, as alſo AD and BC, the ſquare of BD is equal to the ſquare of BC, together with the rectangle under AB and DC.

Now let the point B repreſent the moon's apparent place, and D the apparent place of the ſun or a ſtar, and let the chords AB and DC be parallel to the horizon; then is BD the chord of the arc of the apparent diſtance, and AD or BC are the chords of the arcs of azimuth circles paſſing through the ſun or a ſtar and the moon, intercepted between the parallels of apparent altitude; or in other words, AD or BC is the chord of the difference of altitude. The length of the chord AB is to that of DC in the ratio of the coſine of the altitude of the object at B to the coſine of the altitude of the object at D, and the rectangle under the chords AB and DC will be to the rectangle under any two aſſumed chords parallel to AB and DC, and terminated by the ſame azimuth circles which paſs through the points B and D, as the rectangle under the coſines of the altitudes of the points B and D is to the rectangle under the coſines of the altitudes of the two aſſumed chords. And ſince BD and BC are had by obſervation, the rectangle under AB and DC is alſo known, being equal to $BD^2 - BC^2$, or to $\overline{BD + BC} \times \overline{BD - BC}$.

Make b the moon's true place, ſituated ſomewhere in the ſame azimuth circle that paſſed through B, and d the ſun or ſtar's true place, ſituated in the ſame azimuth circle that paſſed through D; and analagous to A B C D draw the quadrilateral Figure $a\,b\,c\,d$, which will have all the properties of the former, and ad or bc will be the chord of the arc of the difference of the true altitudes, and bd the chord of the arc of the true diſtance. And it has been already ſhewn, that the rectangle under the coſines of the apparent altitudes is to the rectangle under the coſines of the true altitudes, as AB × DC is to

a b

$ab \times dc$, wherefore the rectangle under ab and dc being found and added to the ſquare of bc or ad, which is ſuppoſed to be known, the ſum will be equal to the ſquare of bd, the chord of the true diſtance, which may conſequently be determined.

Theorems for clearing the diſtance of parallax and refraction.

If the half chords, or ſines of half the arcs be uſed inſtead of the chords of the whole arcs, the true diſtance will, from the foregoing principles, be obtained by the following theorems.

$$1.\ \text{Sine}^2\left(\frac{\text{app. diſt. ☽ \& } \odot \text{ or } *}{2}\right) - \text{ſine}^2\left(\frac{\text{diff. app. alt. ☽ \& } \odot \text{ or } *}{2}\right) = Q$$

$$2.\ \frac{\text{Cos. ☽'s true alt.}}{\text{Cos. } \odot\text{'s app. alt.}} \times \frac{\text{Cos. } \odot \text{ or } *\text{'s true alt.}}{\text{Cos. } \odot \text{ or } *\text{'s app. alt.}} \times Q = q$$

$$3.\ \text{Sine}^2\left(\frac{\text{diff. true alt. ☽ \& } \odot \text{ or } *}{2}\right) + q = \text{ſine}^2\left(\frac{\text{true diſt. ☽ \& } \odot \text{ or } *}{2}\right)$$

Tables neceſſary.

If a table of chords, or of natural ſines of the 2d power, were conſtructed, the ſquare numbers correſponding to certain given arches, and converſely, the arches correſponding to certain given ſquare numbers, to be employed in the ſolution of the above theorems, might be had without farther trouble, by inſpection. A ſecond table might alſo be conſtructed ſo as to fit the 2d theorem, and ſimplify the calculation for finding q, but this happens to have been already done, as Table IX, with the ſhort, additional, corrective Tables X. and XI. in the 3d edition of the requiſite tables fully anſwer this purpoſe, by readily furniſhing a logarithm, which added to that of Q gives the logarithm of q. The nature of theſe tables is explained near the end of that work.

Advantages of the method.

The method now propoſed of ſolving the problem of the longitude, ſo far as a ſolution depends on finding the true diſtance between the moon and the ſun, or any given fixed ſtar, has ſeveral advantages, it is conceived, over the common methods, as the anſwer will in every poſſible caſe, be determinate; the diverſe affections of the ſides of the ſpherical triangles commonly uſed never occaſioning in this way any embaraſſment to the operator, and as there never happens any change of ſigns from + to −, or from − to +, in any part of the calculation, it is plain the operation muſt always be clear of ambiguity. It will alſo be found on trial, that the nature of the numbers employed in this mode of calculation tends very much to preclude ſmall errors, or to render their effects inſenſible. A table of logarithms of natural numbers, a table of the ſquares of chords, and Table IX. with its corrections

from

from the requifite tables, are all the tables neceffary (after having found the moon and the fun or ftars true altitude) according to the new method, for finding the true diftance required. And as they require to be opened only about feven times, the whole operation might be eafily performed in about five minutes; and if carefully done, the anfwer will not vary one fecond of a degree from the truth.

EXAMPLES.

Examples. 1. Given the apparent altitude of the moon's center, 35° 49.57, and the true altitude 36° 33′ 9″; the apparent altitude of the fun's center 28° 49′ 54″, and true altitude 28° 48′ 19″, and the apparent diftance of their centers 95° 31′ 2″ req^d the true diftance.

	App. Alt.		Log. Cofines.		True Alt.		Log. Cofines.
	° ′ ″				° ′ ″		
☽	35 49 57	–	9.9088773	☽	36 33 9	–	9.9048840
⊙	28 49 54	–	9.9425241	⊙	28 48 19	–	9.9426341
	2) 7 0 3		19.8514014		2) 7 44 50	+	19.8475181
	3 30 1½	Nat. fine²	0037278		3 52 25	–	19.8514014
½ App. dift.	47 45 31	Nat. fine²	5480725			–	0.0038833
		Difference	5443447		——— Log.	+	9.7358740
					5394990	=	9.7319907
½ Diff. true alt.	3 52 25	N. S.²			0045638		
½ True dift.	47 31 40	N. S.²			5440628		
	2						
	95 3 20	=	True diftance.				

2d. Example from the Requifite Tables, page 38, 3d Edition, *ad fin.*

App. alt. *	° ′ 11 17		True alt. *	° ′ ″ 11 12 21	
— — ☽	9 38	Ho. Par. 54′ 42″ — — ☽		10 26 28	
	2) 1 39			2) 0 45 53	
	0 49 30	N. S. 0142530		0 22 56½	L. S. 7.8243478
½ App. dift.	21 47 51	N. S. 3713273			2
Log. 9.5861148	=	3855803	= S.	0000445	= 5.6486956
Log. 9.5527587	=	3570743	= D.	1373746	
Tab. IX. Log. 9.9990330				1374191	= 19.1380470
19.1379065	=	1373746		° ′ ″ 21 45 31,8	= 9.5690235
				2	
				43 31 2½	= True diftance

3. Example

3. Example from the Requisite Tables, page 41 & 43, 3d Edition, *ad fin.*

App. alt. * 24° 48′   True alt. * 24° 45′ 57″
— — ☽ 12 30 Ho. Par. 56′ 15″ — — ☽ 13 20 42
2) 12 18   2) 11 25 15
6 9 Nat. sine 1071318   5 42 37½ L. S. 8.9978259
½ App. dist. 25 44 17½ — 4342618   2
Log. 9.7335131 = 5413936 = S.   0099004 = 7.9956518
Log. 9.5147204 = 3271300 = D.   1765564
Tab. IX. Log. 9.9986500   1864568 = 19.2705783
19.2468835 = 1765564   Sine 25 34 56 = 9.6352891
2
True distance = 51 9 52

4. July 6th, 1790, at 9h 13′ 15″ A. M. apparent time, the apparent altitudes and distance of the sun and moon's centers were observed as follows: Moon's alt. 25° 29′ 11″, Sun's alt. 15° 54′ 5″ distance 66° 19′ 32″, Moon's Ho. Par. 54′ 35″ reg^d the true distance.

App. alt. ☽ 25° 29′ 11″   True alt. ☽ 26° 16′ 28″
⊙ 15 54 5   ⊙ 15 50 55
Difference 9 35 6 Tab. II. 0069802   10 25 33 Tab. II. 0082550
App. dist. 66 19 32 Tab. II. 2992304   2903825
Log. 9.4657548 = 2922502 T. dist. 66 15 4 Tab. II. 2986375
Tab. IX. Log. 9.9972157
9.4629705 = 2903825

Note.—In the three first examples the computation is carried on by the help of the tables that are in common use, but in this last example, by referring to the proposed table of the squares of chords, under the title of Table II. some idea may be had of the labour required in the more improved method of computation. There is nothing farther to add than that, if after the many modern improvements in the way of clearing the observed lunar distances of the effects of parallax and refraction, the present mode should be thought of utility, and worthy of adoption, the necessary tables, or at least that part which is new, will be published in a separate octavo volume.

Analysis

X.

*Analyfis of the Propolis or Maftic of Bees.** By CIT. VAUQUELIN.

Propolis or bee maftic.

THE propolis is well known to be, the firft matter collected by the bees of a fwarm, newly placed in a hive. This fubftance is refinous, ductile, adorant and of a reddifh brown colour, more or lefs deep.

The propolis adheres fo ftrongly to the legs and feet of the bee, which has collected it, that it is not able to free itfelf from it. Other working bees, to whom this bee feems to prefent its legs, carry off, with their jaws, this tenacious matter, and apply it round the infide of the hive, particularly over all the projections: hence this name of propolis, which fignifies in the Greek *before the city*.

Its general properties and application.

This propolis is at firft foft and very extenfible; but it hardens, and at laft becomes very folid. It is not yet known from what part of plants it is extracted: it is believed that it comes from that fpecies of gum-refin which coats and defends moft of the buds of trees and fhrubs from wet. By continuing this work withinfide the hive, all the foreign bodies, which are introduced into the common habitation and are too heavy to be removed to the outfide, are covered over with this matter, and thus put out of the boundary.

The following are the obfervations made by Cit. Vauquelin on this fubject, and which he has inferted in a report made to the Society of Agriculture, in concert with Cit. Lafteyrie.

In the mafs it is blackifh; but it is femitranfparent when in thin plates. The heat of the fingers is fufficient to foften it and give it all the ductility of wax: but it is more ropy and tenacious. Like wax it may be kneaded between the teeth, without any perceptible tafte. Its odour is aromatic, refembling that of melilot, of balfam of Peru, or of the Banana poplar.

Analyfis: Solution in alcohol: filtration.

One hundred grammes of this fubftance were digefted, for twenty-four hours, in very pure alcohol. The liquor acquired a deep red tint: and was thus filtered. Frefh fpirit of wine was put to the refidue, and left to digeft again in the cold during twenty-four hours. As it had gained but little colour,

* Abridged in the Bulletin de la Société Philomathique, from the Memoirs of the Society of Agriculture of the Department of the Seine.

 a third

a third quantity of alcohol was added to the marc, then boiled for a few minutes: and filtered quite hot, Six portions of alcohol were fucceffively added to the marc and boiled: finally to deprive it of the fat matters it retained from fragments of bees, as well as fome vegetable fubftances and grains of fand, boiling fulphuric ether was poured on it, and the mafs was preffed through a fine ftrainer. The refidue dried, weighed 14 grammes.

To obtain the fubftance which had been diffolved by the alcohol, the whole quantity that had been employed in the different wafhings was collected, and paffed through a fine ftrainer, which ftopped all the matter precipitated by the cold. This refidue was wrapped in unfized paper and preffed; when dried and melted, it weighed 14 grammes. Precipitate by cooling.

All the alcohol was then diftilled and reduced to three-fourths of its quantity. The liquor which came over had an aromatic odour; but it did not render water turbid, nor was it acid. What remained in the retort was of a deeper colour. Its precipitate by water was ropy like the refins which are obtained by the fame means. By diluting this liquor with water and boiling it, a refiniform mafs was obtained on cooling, of a red brown colour, femi-tranfparent and very brittle, which weighed 57 grammes. The water in which this matter had been diffolved, contained an acid. Diftillation gave aromatic fpirit. Three fourths which remained in the retort gave a refinous precipitate by water. The water was acid.

This refinous mafs, or pure propolis melts readily on the fire: it yields by diftillation a volatile oil, white and of a very agreeable fmell. The fixed part then acquires a deeper colour and becomes harder: it is foluble in fixed and volatile oils. It is a true refin, very fimilar to balfam of Peru, of which it contains the acid. Properties of the refin.

The 14 grammes of precipitate produced by cooling, were true wax, poffeffing all its properties. It remains to be known whether this wax is actually mixed with the propolis by the bees; or whether, in collecting the latter with too little care the wax is not united to the refin. The firft precipitate was wax.

Component parts.

	grammes.
Pure wax - - - - -	14
Pure refin of propolis - -	57
Refiduum of extraneous bodies	14
Lofs; acid; aroma, - -	15
	100

XI.

Memoir on the cutaneous aeriform Tranfpiration. Read at the public Sitting of the Society of Health, at Grenoble, the 4th Frimaire, in the year 11. *By* CIT. TROUSSET, M. D. *etc.**

Introductory obfervations.

PHYSICIANS have in all ages endeavoured to afcertain the influence of the air on the human body; but how it is to be conceived that the ancients, who were not even acquainted with the gravity of that fluid, fhould be able to determine its action? For the fame reafon, if we except Hippocrates, who in his work fays plainly that the air is digefted in the lungs, like the food in the ftomach, his cotemporaries and their fucceffors have only left behind them, on this fubject, incoherent fancies, often ridiculous, and always wrong; the offspring of imaginations undirected by any certain experience.

If it were my intention to confider the influence of the air, in all its relations, I fhould firft attend to refpiration, a function of fuch importance, that without it life could not be fuftained, while alone it would require fome time for its difcuffion.

But modern chemifts, after having made an accurate analyfis of atmofpheric air, have formed a theory of refpiration, fo ingenious, fo complete, and founded on fuch exact experiments, that every effort which has hitherto been made to overturn it has only ferved to render it more folid.

I could, then, only repeat here what has been already faid by Lavoifier, Seguin, Crawford, Fourcroy, Chaptal, &c. for this fubject has been fo well elucidated by the labours of thefe celebrated chemifts, that it may be faid to be exhaufted; and their doctrine, on this topic, has been fo diffeminated, that, at prefent, it is known and adopted by all who employ themfelves in fcientific purfuits; I fhall, therefore, difpenfe with a repetition that would be faftidious and ufelefs.

Whether gas be emitted from the fkin.

But though the operations of the lungs are very accurately known, the functions of the fkin have not been fo diftinctly elucidated. Independent of the cutaneous tranfpiration fo carefully and diligently obferved by Sanctorius and many others, we may enquire whether one or feveral aeriform fluids efcape

* Annales de Chimie, XLV. 73.

by

by the ſkin? And if ſo, what may be their nature? Theſe are the queſtions I propoſe to examine in the preſent memoir.

The ancients did not even ſuſpect the cutaneous tranſpiration of air, and make no mention of it in any of their works.

C. de Milly firſt obſerved it.

The count de Milly firſt announced, in 1777,* the diſcovery of an elaſtic fluid which paſſes off by the ſkin. He aſſerts that, being in a warm bath, half a pint might have been collected in the ſpace of three hours; and from his analyſis, which was both inaccurate and very incomplete, he concluded that it was fixed air (carbonic acid gas.)

D. Ingenhouz.

M. Ingenhouz, ſome time afterwards noticed this tranſpiration of air by the ſkin, but he believed it to be phlogiſticated air. (nitrogen gas.)

Prieſtley, Fontana.

M. M. Prieſtley and Fontana repeated the experiments of the two preceding philoſophers, but could not perceive any aeriform emanation from the ſkin.

Jurine.

M. Jurine, ſurgeon at Geneva, intending to be a competitor for the prize propoſed by the Royal Society of Medicine,† repeated the experiments of M. M. Milly and Ingenhouz, not only on himſelf but on ſeveral individuals of all ages, uſing different kinds of water and varying the temperature; he informs us that he never diſcovered any emanation of air. Preſuming that the water, by its preſſure might have obſtructed the paſſage of the air; or that it contracted the exhaling air-veſſels of the ſkin, he continued his inquiries, by varying the proceſſes before uſed by M. M. Prieſtley and Fontana, and he thought he had proved by experiments, the inaccuracy of which may be eaſily ſhewn, that a ſmall quantity of fixed air (carbonic acid gas) is conſtantly emitted by the ſkin.

Fourcroy.

Cit. Fourcroy expreſſes himſelf thus: "it is not true, as ſome moderns have aſſerted, that elaſtic fluids and particularly carbonic acid gas are tranſpired by the ſkin" *.

Such, a very few years ago, was the ſtate of the queſtion which now engages us. Incorrect experiments, the contradictory reſults of which were diſputed either wholly or in part,

* Memoirs of the Royal Academy of Sciences at Berlin, for 1777, p. 32.

† See Hiſtory and Memoirs of the Society of Medicine, Vol. X. p. 54, and fol.

* Fourcroy, Syſtem of Chemical Knowledge, Vol. IX. p. 203.

 left

left philofophers in uncertainty, and appeared to require new obfervations, in order that the opinions of chemifts might be finally fettled on this fubject.

Obfervation of gas emitted from the fkin in the bath.

I had often meditated on this fubject with an intention of engaging in it, when in the fpring of the year eight, while attending one of my patients in the bath, I perceived that he was entirely covered with bubbles of air; the hairs on his body were furrounded with bubbles decreafing from the bafe to the fummit, fo that many of them prefented the appearance of pyramids more or lefs elevated. I difplaced all thefe bubbles, and in half an hour afterwards there appeared an equal quantity.

It was nitrogen.

On witneffing this phenomenon, I engaged my patient to continue the bath, and collected fucceffively feveral jars of this gas, which I examined with much care and at different times. I was fatisfied that it was perfectly pure nitrogen gas, without any mixture of carbonic acid.

I wifhed then to know whether this phenomenon was general, or depended on the pathologic ftate of the fubject. Experiments made on myfelf and on feveral other individuals did not offer any thing fimilar.

I communicated my experiments and their refults to Cit. Fourcroy, in the beginning of the year nine, who encouraged me to repeat them. Stimulated by the approbation of this celebrated profeffor, I undertook the tafk. It was not enough for me, in fact, to be certain of the accuracy of my experiments, it was alfo neceffary to convince others.

Detail of experiments on this gas.

In the fpring of the year nine, having collected a certain quantity of this gas with great care, I filled a fmall glafs jar 10 lines in diameter, and eight inches high, with it; a taper was inftantly extinguifhed in it eleven times fucceffively.

I endeavoured to decant a fimilar meafure of this gas, but could not fucceed, which I could eafily have done had it been carbonic acid.

It was paffed through lime-water without rendering it turbid or diminifhing in volume.

It underwent no change with ammoniacal gas.

It did not affect blue vegetable colours.

Phofphorus which had remained immerfed in it for more than a month, was fcarcely diminifhed in bulk.

I judged

I judged thefe experiments to be more than fufficient to fhow that it was nitrogen gas.

I again tranfmitted the refult of thefe new experiments to Cit. Fourcroy, in Fructidor of the year nine; probably his occupations have prevented him from fending me any reply.

Having fince that period reflected much on the importance of this difcovery, I have continued to attend to it, but my notions are entirely changed with regard to the confequences to be drawn from it.

The emiffion of gas from the fkin confidered as a general phenomenon.

Formerly in the letters written to Profeffor Fourcroy, and efpecially in the latter, I confidered as a particular fact, dependant on the pathologic ftate; at prefent I am compelled to confider it as a general phenomenon belonging to the whole human fpecies.

1ft. Becaufe it is probable that the gas fo abundantly yielded by the fkin of M. le Comte de Milly, was nitrogen gas, as may be feen by an attentive perufal of his memoir.

2d. M. Ingenhouz, convinced by his own experiments, that an aeriform fluid is emitted by the fkin, believed it to be phlogifticated gas (nitrogen gas.)

3d. My experiments confirm the fufpicion of M. Ingenhouz.

4th. Some time ago I met with another perfon who tranfpired abundantly in the bath; the bubbles with which he is conftantly covered are not diffolved in the water, it is probabably nitrogen gas, I acknowledge, however, that I have never made any exact experiment on it.

5th. The experiments made by M. M. Prieftley, Fontana and Jurine, which confifted in placing open veffels under their arm-pits, do not prove any thing in oppofition to what I have before advanced, becaufe it is evident that thefe veffels being full of atmofpheric air, it could not be difplaced by nitrogen gas, whofe fpecific gravity is lefs; while this effect would have taken place with carbonic acid gas, if, as M. Jurine infers from his experiments, that this laft fluid is conftantly difengaged by the fkin.

Thofe made by M. Jurine, by placing his arm in a glafs cylinder, are not more conclufive, becaufe they were performed with a view to prove that it was carbonic acid gas which efcaped, and the proper means were not taken to point out the prefence and the quantity of nitrogen gas, whofe difengagement he did not even fufpect.

But

The water supposed to obstruct air in most persons.

But why was not this phenomenon, which I believe to be general, observed by M. M. Prieſtley, Fontana, Jurine, &c. and why is it not equally perceptible in all individuals placed in the ſame circumſtances? It is probable, as I have ſaid in the beginning of this memoir, after M. Jurine, that the water acting by its weight upon the exhaling air-veſſels of the ſkin differently upon different ſubjects, may obſtruct the paſſage of all gaſeous bodies in the greateſt number of individuals.

I ſhall here conclude this memoir, which I might have extended much farther, had I been inclined to diſcuſs all the queſtions to which the phenomenon naturally gives birth; but having had no other deſign but to call the attention of natural philoſophers to the ſubject, and ſatisfied with having began the reſearch, I leave the completion to more ſkilful chemiſts.

SCIENTIFIC NEWS.

Decree of the French Government relative to the new Organization of the National Inſtitute.

New organization of the French inſtitute.

THE government of the republic, upon the report of the miniſter of the interior, having adviſed with the counſel of ſtate, decree as follows:

I. The national inſtitute at preſent divided into three claſſes, ſhall from henceforth be divided into four; that is to ſay,

Firſt Claſs. Claſs of phyſical and mathematical ſciences.

Second Claſs. Claſs of French language and literature.

Third Claſs. Claſs of hiſtory and foreign literature.

Fourth Claſs. Claſs of the fine arts.

The actual members and foreign aſſociates of the inſtitute, ſhall be divided into theſe four claſſes.

A committee of five members of the inſtitute, named by the Firſt Conſul, ſhall adjuſt this diviſion, which ſhall be ſubmitted to the approbation of government.

II. The firſt claſs ſhall be formed of the ten ſections which at preſent compoſe the firſt claſs of the inſtitute, with a new ſection of geography and navigation, and eight foreign aſſociates.

The

The fections fhall be compofed and called as follows:

New organization of the French inftitute.

Mathematical Sciences	Geometry	6 members.
	Mechanics	6 ditto.
	Aftronomy	6 ditto.
	Geography and Navigation	3 ditto.
	General Phyfics	6 ditto.
Phyfical Sciences	Chemiftry	6 members.
	Mineralogy	6 ditto.
	Botany	6 ditto.
	Rural Economy and Veterinary Art	6 ditto.
	Anatomy and Zoology	6 ditto.
	Medicine and Surgery	6 ditto.

The firft clafs fhall appoint, with the approbation of the Firft Conful, two perpetual fecretaries, one for the mathematical fciences, the other for the phyfical fciences. The perpetual fecretaries fhall be members of the clafs, but fhall not form a part of any fection.

The firft clafs may elect fix of its members from the other claffes of the inftitute.

It may name an hundred correfpondents felected from the learned men of France and foreign countries.

III. The fecond clafs fhall be compofed of forty members.

It is particularly charged with the compofition of the dictionary of the French language. It fhall examine with refpect to language, the important works of literature, hiftory and the fciences. The collection of its critical obfervations fhall be publifhed at leaft, four times in a year.

It fhall name from its own body, and with the approbation of the Firft Conful, a perpetual fecretary, who fhall continue to be of the number of the forty members which compofe it.

It may elect twelve of its members from the other claffes of the inftitute.

IV. The third clafs fhall be compofed of forty members and eight foreign affociates.

The objects of its refearches and labours fhall be learned languages; antiquities and monuments; hiftory and all the moral and political fciences connected with hiftory. It fhall particularly apply itfelf to the enriching of French literature with the works of Greek, Latin and Oriental authors, which have not yet been tranflated.

It fhall employ itfelf in the continuation of diplomatic collections.

It

New organization of the French institute.

It ſhall name from its own body, under the approbation of the Firſt Conſul, a perpetual ſecretary, who ſhall be of the number of the forty members which compoſe the claſs.

It may elect nine of its members from the other claſſes of the inſtitute.

It may name ſixty correſpondents, natives or foreigners.

V. The fourth claſs ſhall be compoſed of twenty-eight members and eight foreign aſſociates.

They ſhall be divided into ſections, deſcribed and formed as follows:

Painting - - - - -	10 members.
Sculpture - - - - -	6 ditto.
Architecture - - - -	6 ditto.
Engraving - - - - -	3 ditto.
Muſic (compoſition) - - -	3 ditto.

It ſhall appoint, with the approbation of the Firſt Conſul, a perpetual ſecretary, who ſhall be a member of the claſs, but ſhall not be a part of a ſection.

It may elect ſix of its members from the other claſſes of the inſtitute.

It may name thirty-ſix correſpondents, national or foreign.

VI. The foreign aſſociated members ſhall have a deliberative voice only on ſubjects of ſcience, literature and the arts; they ſhall not form part of any ſection, nor interfere in any uſage.

VII. The preſent actual national aſſociates of the inſtitute ſhall form part of the one hundred and ninety-ſix correſpondents attached to the claſſes of the ſciences, *belles lettres*, and fine arts.

Correſpondents may not aſſume the title of members of the inſtitute.

They ſhall loſe that of correſpondent when they ſhall be domiciliated at Paris.

VIII. Nominations to vacant places ſhall be made by each claſs in which the vacancy happens, the perſons elected ſhall be confirmed by the Firſt Conſul.

IX. The members of the four claſſes ſhall enjoy a reciprocal right to aſſiſt at the particular ſittings of each claſs, and may deliver lectures when they are requeſted.

They ſhall re-unite four times in a year into one body, to communicate their proceedings.

They

They ſhall elect in common the librarian and under librarian of the inſtitute, as well as all thoſe agents who belong to the inſtitute in common.

New organization of the French inſtitute.

Each claſs ſhall preſent for the approbation of the government, the particular ſtatutes and regulations of its internal police.

X. Each claſs ſhall hold one public ſitting every year, at which the other three ſhall aſſiſt.

XI. The inſtitute ſhall receive annually from the public treaſury 1,500 fr. for each of its non-aſſociated members, 6,000 fr. for each of its perpetual ſecretaries; and for its expences, a ſum which ſhall be fixed every year, upon the demand of the inſtitute, and compriſed in the eſtimates of the miniſter of the interior.

XII. There ſhall be an adminiſtrative committee for the inſtitute, compoſed of five members, two from the firſt claſs, and one from each of the others, named by their reſpective claſſes.

This committee ſhall regulate in the general ſittings preſcribed by Art. IX. all that relates to the adminiſtration, to the general expences of the inſtitute, and to the diviſion of its funds between the four claſſes.

Each claſs ſhall afterwards regulate the application of the funds aſſigned to it for its expences, as well as all that concerns the printing and publiſhing its memoirs.

XIII. Every year the claſſes ſhall diſtribute prizes, the number and value of which ſhall be regulated as follows:

The firſt claſs, a prize of 3,000 francs.

The ſecond and third claſs, each a prize of 1,500 francs.

The fourth claſs, grand prizes of painting, ſculpture, architecture and muſical compoſition. Thoſe who ſhall have gained one of the grand prizes, ſhall be ſent to Rome, and maintained at the expence of government.

The above decree was followed by a ſecond, appointing the members of the different claſſes, and regulating the days of their meeting.

The ſittings of the firſt claſs are to be held every Monday; thoſe of the ſecond claſs every Wedneſday; thoſe of the third every Friday; and thoſe of the fourth every Saturday.

The ſittings are to be held in the ſame place, and to laſt from three o'clock till five.

New organization of the French institute.

The firſt ſitting of the firſt claſs, in Vendémiare; that of the ſecond, in Nivoſe; that of the third, in Germinal; and that of the fourth, in Meſſidor; are to be public.

In conformity to theſe decrees, the national inſtitute has been re-organiſed.

The firſt claſs, that of phyſical and mathematical ſciences, held its firſt ſitting on Monday the 11th Pluvioſe.

C. Chaptal was choſen preſident; and C. Cuvier, perpetual ſecretary of the ſection of phyſical ſciences; and C. Delambre, for that of mathematical ſciences.

The committee to propoſe regulations are CC. Laplace, Monge, and Fourcroy.

The ſitting of the ſecond claſs, that of French language and literature, was held on Wedneſday the 13th.

Preſident: C. Lucien Bonaparte. Perpetual ſecretary: C. Suard.

Committee, &c. CC. Morallet, Bigot-Preameneu and Andrieux.

The third claſs, that of hiſtory and foreign literature, on Friday the 15th.

Preſident: C. Le Brun. Perpetual ſecretary: C. Dacier.

Committee, &c. CC. Paſtoret, Dutheil, and Sylveſtre de Sacy.

The fourth claſs, that of the fine arts, on Saturday the 16th.

Preſident: C. Vincent. Perpetual ſecretary: C. Labreton.

Committee, &c. CC. Vincent, Danon, Dufourny, Gretry and Moitte.

Galvanic Information.

Galvanic reſearches.

THE *Moniteur* of the 2d Floreal (April 22,) ſays "the galvanic ſociety continues its labours with zeal and with the moſt marked ſucceſs."

"Cit. Gautherot has performed a ſucceſſion of experiments tending to prove that the developement of electricity is proportionate to the ſurfaces.

"New diſcoveries have been publiſhed by Profeſſor Aldini relative to the exiſtence of a galvanic atmoſphere, and to the contractibility of the heart.

"Cit.

"Cit. Nauches, the prefident, with his co-labourers Bonnet and Pajô-Laforêt, have fucceeded, by the affiftance of two *homogeneous* metallic conductors, in fubtracting the electric fluid of the brain, and fpinal marrow of an ox, immediately after its death, and conveying it to the thighs of a frog, where it produced mufcular contractions. This operation was equally fuccefsful upon the palpitating mufcles, and could not be prolonged beyond a quarter of an hour after death. The fenator Lamartilliere has given an explanation of the difengagement of mucofity by one of the poles of the pile, and has made it appear that depends on a chemical decompofition.

Cit. Paroiffe took notice that, of the mufcles, the diaphragm preferved its galvanic excitability the longeft.

Cit. Izaer gave an account of the conftruction of a pile, difcovered by Cit. Alizeau, in which the roundels, impregnated with a faline folution, are replaced by moiftened falt, and which preferves its power for a month, without being cleaned.

The committee of medical application, confifting of M. M. Guillotin, Dudaujon, Petit-Radel, &c. have made a number of experiments on *afphixia* produced by ftrangulation.

"The application of galvanifm to the cure of difeafes which had been fufpended on account of the unfavourable feafon, is to be refumed in a ward which has been appropriated, by the minifter of the interior, to that purpofe, in the hofpital of the *Ecole de Medicine*, and in a laboratory belonging to the fociety."

Prize Queftion.

The Society of Arts and Sciences at Utrecht, has propofed the following Prize Queftion for the Year 1803. **Prize queftion. Electricity.**

"What is the true nature of the electric matter? Is it a compound? What are the chemical changes it undergoes, when united with other bodies, and which it produces in thofe bodies?"

The prize, which is to be adjudged on the firft of October, 1803, confifts of 30 ducats; and the memoirs are to be addreffed to Dr. Luckman, at Utrecht.

Hermaphrodite

Hermaphrodite Fish.

Hermaphrodite fish.

In Vol. XXI. of the "*New Transactions of the Royal Academy of Sciences at Stockholm,*" is an account of an eel (gadus lota) in which eggs and soft roe were found at the same time. It is by professor J. G. Pipping.

Experiment calculated to prove that the Laws of Galvanism appear to differ from those of Electricity. By LAGRAVE, *Member of the Galvanic Society* *.

Supposed difference between the electric and galvanic fluids.

The experiments which I submit to the public have appeared to me to differ from the laws of electricity, in this respect, that the galvanic fluid seeks the most intimate affinities, and that electricity appears to endeavour to escape at the first touch.

My Experiments were as follows:

Experiments with a long conductor.

I raised a pile of seventy pair of plates, of copper and zinc, perfectly free from oxidation; I then applied, to the zinc end, a conductor six ells long, which I passed once round my arm, then twice round my body, above my cloaths, from whence I conducted it round my thigh and my leg, also once round each, and secured its lower extremity to my foot, in a bucket of warm water: I wetted my right hand, (the great conductor was round my arm, thigh and leg, on the left side) and having brought my fore-finger to the copper end, I experienced the most lively shock in this hand and arm, but felt nothing from the great conductor of the zinc end. Surprized not having had the slightest sensation from this pole, I repeated the experiment with the same effect; and at first believed that, in this case, the galvanic fluid was governed by the laws of electricity; that is to say, that it was disposed to pass off by the shortest way; but I quickly discovered another cause, when, on taking a copper wire, two feet long, in my right hand, and placing it in contact with the copper pole, I felt the shock in the foot immersed in the bucket of water. I was equally astonished at receiving the shock in my foot, notwith-

Galvanic shock perceived in the moistened hand;

in the foot, when the circuit was made with a second *dry* conductor,

* From the Journal de Physique, Ventose, An. XI.

withstanding

notwithſtanding the wire was ſo repeatedly wound round my arm, my body, my thigh, and my leg, without yielding, in this long courſe, the ſlighteſt effect: I repeated this experiment with the ſame ſucceſs; and ſoon diſcovered that the ſhock I received in my foot, to the excluſion of the other points of contact, was only occaſioned by the affinity of the water for the galvanic fluid, and becauſe it ſoftened the epidermis: but that which convinced me of it, was, that on putting drops of water alternately, on the turns of the wire, and then bringing that connected with the copper pole in contact with it, I inſtantly felt the ſhock at the places where the drops of water were put, and experienced no ſenſation in my foot. I repeated this experiment, putting a drop of water upon the ſeveral turns of the wire, and connecting the copper pole with it: ſimilar effects were produced without my perceiving the ſlighteſt ſenſation lower down; although I had neglected to wipe off the moiſture occaſioned by the different drops of water. I obſerve, in this operation, the rule which governs electricity, that is to ſay, that it is diſpoſed to give the ſpark by the ſhorteſt way; but, on the other hand, I alſo find, that the galvanic fluid is inclined to ſeek an affinity which does not ſeem to agree with the electric fluid; becauſe, we obtain a ſpark from the firſt contact, but here, the galvanic fluid paſſes through a conſiderable ſpace of the moſt intimate points of contact, without loſing any of its power, and produces no ſenſation until it has found an affinity favourable to its diſengagement. I felt alſo the moſt diſtinct effects, on taking a wire of great length in one hand, with which I touched the poſitive pole, as in the former experiment I made a long circuit with it entirely round my room, and having brought back the end to my left hand, I put the right in contact, and felt a ſimilar ſhock in my right hand, but if I took a wire of two feet in that hand, without being careful to wet it as well as the other, I felt nothing. The fluid paſſed through the long circuit of the wire to reach that point which offered the greateſt affinity; thus I placed a conductor at either pole, and uniformly felt the ſhock from that pole which I touched with my naked and moiſtened hand.

and in various parts, in contact with the great conductor, by placing drops of water on it.

Concluſions.

Such were my obſervations on this ſeries of experiments.

It will be the greateſt ſatisfaction to me, if they ſhould give riſe to ſuch clear opinions as may tend to the good of humanity and to the progreſs of the arts and ſciences.

Galvanic

Galvanic Experiments, tending to ascertain the Existence of Two Fluids in the Animal Economy, the one positive and the other negative, which, by their Union, appear to produce the Agency of Vitality, by La Grave, *Member of the Galvanic Society* *.

Galvanic pile formed of muscle and brain with cloth interposed.

BEING desirous of satisfying myself concerning the phenomena which are exhibited in the experiments made on frogs, by bringing part of each of their thighs in contact upon the cervical nerves, and persuaded that this fact could only proceed from the concurrence of two fluids, the one positive and the other negative, as we find it in the pile of Volta, I felt inclined to form a pile, composed of the nervous and muscular parts alternately. With this view, I procured a body, and prepared and uncovered, as much as possible, a certain number of the pectoral and intercostal muscles, which I cut into the form of discs; I then took the brain, not being able in this experiment to make use of the nervous cords, and cut, as thin as I could, the same number of discs as I had prepared from the muscular parts. Of these I raised a pile to the number of fifteen or sixteen couple, placing between the pairs, pieces of cloth moistened in salted water. It was in vain that I endeavoured to raise my pile with the pieces of cloth; the soft parts of the brain and their flexibility prevented it. I attempted it several times, because the experiment appeared to me to be curious, but I could not succeed. Being still desirous to overcome these difficulties, it occurred to me that pieces of leather or of hat might fulfil my wishes, as they possess a proper stiffness to support the soft part of the brain. In fact, I tried both, but those of hat soon obtained the preference over the pieces of leather, because they have the quality of being porous, of retaining the moisture better, and of possessing a degree of elasticity, which the leather has not. I then made some fresh preparations of muscle and brain, and raised a pile, making use of discs of hat for separating them; but after having raised my pile to twenty couple, I experienced new difficulties. The cerebral parts were crushed and began to escape from the weight of the pile. I need not observe that I took care to examine whether this small number of pairs was sufficient to produce the taste. I obtained no effect from the

Hat substituted for cloth.

* From the Journal de Physique, Ventose, An. XI.

twentieth

twentieth couple: it was neceſſary therefore to determine to abandon the experiments, or to deviſe ſome other mode. For this purpoſe, I thought of making a layer of ſmall ſtring the length of the pile, tied to the glaſs of the inſulater of the pile of Volta to ſuſtain my rounds and my pairs of the fleſhy and cerebral parts; this idea ſucceeded. I continued then to raiſe my pile upon theſe new ſupports: I was aſtoniſhed and much more impatient to perceive nothing at the thirtieth couple. Obſtinately bent on ſucceeding or being convinced that my opinion was erroneous I perſiſted and continued to heighten my pile; I did not perceive the leaſt effect until I had added the fortieth pair. Encouraged by this ſucceſs, I redoubled my attention and was thoroughly convinced of the exiſtence of the taſte at the fiftieth couple. Neverthelesſ I continued increaſing the pile to the ſixtieth, and the evidence of its effects was moſt deciſive.

Other contrivances.

At the fiftieth pair the taſte was perceived and at the ſixtieth deciſive.

Such were my diſagreeable but curious experiments.

ACCOUNT OF NEW BOOKS.

Tranſactions of the American Philoſophical Society held at Philadelphia for promoting Uſeful Knowledge. Vol. V. 1802, *p. p.* 328. *Quarto.*

American philoſophical tranſactions.

THE contents of this volume are I. Experiments on the miſſion of acids, and other liquids, in the form of vapour, over ſeveral ſubſtances in a hot earthern tube—II. Experiments relating to the change of place in different kinds of air through ſeveral interpoſing ſubſtances—III. Experiments relating to the abſorption of air by water—IV. Miſcellaneous experiments relating to the doctrine of phlogiſton—V. Experiments on the production of air by the freezing of water—VI. Experiments on air expoſed to heat in metallic tubes—VII. Some account of the poiſonous and injurious honey of North America—VIII. On the ephoron leukon, uſually called the white fly of Paſſaick river—IX. Remarks on certain articles found in an Indian tumulus at Cincinnati, and now depoſited in the muſeum of the American philoſophical ſociety—X. A drawing and deſcription of the clupea tyrannus and oniſcus prægustator *(in the Preſent Number)*—XI. A deſcription

defcription of a newly invented globe time-piece—XII. A defcription of the pendant planetarium—XIII. On the ufe of the thermometer in navigation—XIV. Sur les vegetaux, les polypes et les infectes—XV. Memoir on the analyfis of black vomit—XVI. Obfervations on the foda, magnefia and lime, contained in the water of the ocean; fhewing that they operate advantageoufly there by neutralizing acids, and among others the feptic acid, and that fea-water may be rendered fit for wafhing cloaths without the aid of foap—XVII. Defcription of a ftopper for the openings by which the fewers of cities receive the water of their drains—XVIII. A memoir on animal cotton, or the infect fly-carrier *(fee Phil. Journal for April)*—XIX. Note concerning a vegetable found under ground—XX. Aftronomical and thermometrical obfervations, made at the confluence of the Miffiffippi, and Ohio rivers—XXI. Aftronomical and thermometrical obfervations made on the boundary between the United States and his Catholic Majefty—XXII. Obfervations on the figure of the earth—XXIII. Defcription of fome improvements in the common fire-place, accompanied with models offered to the confideration of the American philofophical fociety.——Appendix, I. An account of a method of preventing the premature decay of peach trees. II. Defcription of a method of cultivating peach-trees, with a view to prevent their premature decay; confirmed by the experience of forty-five years, in Delaware ftate and the weftern parts of Penfylvania.

Parkinfon's chemiftry.

The Chemical Pocket Book; or Memoranda Chemica; arranged in a Compendium of Chemiftry, By JAMES PARKINSON, HOLBORN. *The third Edition, with appropriate Tables and Accounts of the lateft Difcoveries.* 1803 *p.* 272. 12*mo.*

THE character of this work is already eftablifhed. In the prefent edition the neweft difcoveries are inferted. The author has adopted the nomenclature of Chenevix.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JUNE, 1803.

ARTICLE I.

A Report of the State of His Majesty's Flock of Fine-wooled Spanish Sheep during the Years 1800 *and* 1801; *with some Account of the Progress that has been made towards the Introduction of that valuable Breed into those Parts of the United Kingdom where fine Clothing Wools are grown with Advantage. By the Right Hon. Sir* JOSEPH BANKS, *Bart. P.R.S. &c. &c* *.

ON the 9th of June, 1800, when his majesty's Spanish flock was shorn, it consisted of 100 ewes and wethers, which produced as follows: State of his majesty's Spanish flock of sheep, June 9, 1800.

Wool washed on the sheeps' back - -	398 lb.
Loss in scowering - - - - -	104
Amount of scowered wool - - -	294

Which produced, when sorted, prime	234 lb.	at 5*s. per* lb.	}	65*l.* 11*s.*
choice	34,	at 3*s.*		
fribbs	26,	at 1*s.* 6*d.*		

* From a printed copy with which I was favoured by Sir Joseph. In our Journal, Vol. IV. p. 289 (Oct. 1800) the reader will find a copy of *a project for extending the breed of fine-wooled Spanish sheep, now in the possession of his majesty, into all parts of Great Britain where the growth of fine clothing wool is found to be profitable*, which was also drawn up and circulated by the same gentleman. The present report may be considered as a sequel to the interesting history, and proposal contained in the former paper. W. N.

Sheep difpofed of to extend the breed, in 1800.

Eight rams and nine ewes were this year difpofed of, which were all that could be fpared from the flock. Two of the rams went into Dorfetfhire, where the breed is much approved by fome fkilful judges of fheep, and feems likely to produce confiderable advantage by croffing with the common fheep of the country.

Mr. Bridge's account of the improved value of fheep and their wool, by croffing with the Spanifh.

Mr. Bridge, of Winford Eagle, communicated this year the refult of an experiment he had made on three kinds of fheep, *viz.* Dorfet, half Spanifh and half Dorfet, and half Spanifh and half Mendip.

He kept thefe fheep from the year 1798, when they were lambed, till February 1800, when they were butchered as fat fheep; and having valued them in June 1798, he found the carcafes of each fort, with two years wool which had been fhorn from them, to yield at that time the following increafe in value:

	l.	*s.*	*d.*
Real Dorfet - - - - - -	4	5	6
Half Spanifh half Dorfet - -	4	3	8
Half Spanifh half Mendip - -	3	19	2

In thefe experiments Mr. Bridge's woolftapler values the Dorfet wool at 1*s.* 2½*d.* a pound, and the half Spanifh wool at 1*s.* 4*d*½. only; but as the Spanifh crofs in both cafes increafed the quantity of wool, and as half Spanifh wool has never, when its value was properly known, been fold for lefs than 1*s.* 9*d.* and generally more than 2*s.* a pound, there can be no doubt that the improvement in value, arifing from the crofs, is in both cafes confiderable.

Mr. Ridgway's ftatement of fimilar refults.

Mr. J. Ridgway, of Upperton, in the parifh of Yazor in Herefordfhire, communicated an experiment, in which two fheep, the one a Ryeland, and the other half Spanifh and half Ryeland, of equal weights, were fed by him together; the half Spanifh fheep produced in a year 2 lb. 12 ozs. more wool and 5 lb. more mutton than the Ryelander. This gentleman, whom his majefty gracioufly permitted to have rams from the Spanifh flock fome years ago, has alfo fhewn by his accounts that the wool of his flock of about 16 fcore fheep, has been fo much increafed both in quantity and in value by the Spanifh crofs, as to have produced nearly twice as much money for each clip after the Spanifh blood was eftablifhed in it, as it ufually did before.

In

In June 1801, the Spaniſh flock conſiſted of 108 ewes and wethers, His majeſty's Spaniſh flock, June 1801.

Which produced in wool, waſhed on the ſheeps' back	397lb.
Loſs in ſcowering - - - - - - -	112
Amount of ſcowered wool - - - - -	285

Which produced, when ſorted, prime, 237lb. at 5*s.* 6*d. per* lb.
choice, 31, at 3*s.* 6*d.*
fribbs, 17, at 1*s.* 9*d.*
} 72*l.* 1*s.* 9*d.*

The wool of the rams and fatting wethers which had been kept ſeparate, was prepared for ſale at the ſame time, and produced in

Wool waſhed on the ſheeps' back - -	220lb.
Loſs in ſcowering - - - - - -	82
Amount of ſcowered wool - - -	138

Which produced, when ſorted, prime, 96lb. at 5*s. per* lb.
choice, 30, at 3*s.* 6*d.*
fribbs, 12, at 1*s.* 9*d.*
} 30*l.* 6*s.*

This year, eight rams and twenty-two ewes were ſold. If the foot rot had not unfortunately damaged the rams very materially, more of them would have been diſpoſed of. It is, however, obſervable, that although the rams that are kept at Windſor, in rich land are occaſionally attacked by this haraſſing diſeaſe, the ewes and wethers that feed on the dry and hilly paſtures of Oatlands have never been ſubject to lameneſs of any kind. Sheep diſpoſed of to extend the breed.

Eleven wethers that had been ſent to the marſhes, in order to try the effect of rich paſture in fattening ſheep of this breed, were ſlaughtered this year by Mr. King of Newgate Market, previous to the Smithfield meeting, which uſually takes place the week before Chriſtmas. Two of the carcaſes were given to perſons who had been uſeful in aſcertaining the value of the Spaniſh breed; the remaining nine were ſold to Mr. Giblet, butcher, in Bond Street, whoſe judgment in ſelecting, and liberality in purchaſing the beſt carcaſes, is well known, both to thoſe of whom he buys, and to thoſe who buy of him. The ſale bill is as follows: Account of the fattening and ſale of eleven wethers.

				£.	s.	d.
1 ſheep,	6 ſtone	6lb.	at 6*s.* *per* ſtone -	2	0	6
1 ditto,	7	0	6*s.* - - -	2	2	0
1 ditto,	6	1	6*s.* - - -	1	16	9
1 ditto,	7	2	6*s.* - - -	2	3	6
1 ditto,	5	6	6*s.* - - -	1	14	6
				9	17	3

 1 ſheep,

					£.	s.	d.
			Brought over,		9	17	3
1 ſheep,	5 ſtone	2 lb.	at 6*s*. *per* ſtone	-	1	11	6
1 ditto,	5	7	6*s*.	- - -	1	15	3
1 ditto,	5	4	6*s*.	- - -	1	13	0
1 ditto,	6	2	6*s*.	- - -	1	17	6
11 heads and plucks, at 1*s*.				- - -	0	11	0
10 ſtone 4 lb. fat, at 3*s*. 10*d*.				- - -	2	3	0
					£. 19	8	6

Reſpecting the goodneſs of the mutton, enquiry muſt be made of Mr. Giblet, at whoſe ſhop the carcaſes were ſhewn for ſeveral days, and of his cuſtomers who purchaſed the joints. Experience has, however, demonſtrated already, both at Windſor and at Weybridge, that Spaniſh mutton is of the beſt quality for a gentleman's table.

Sale of their pelt wool.

The pelt wool of theſe 11 ſheep was taken off, in order that its value might be aſcertained.

It weighed in the yoke - - - -	36 lb.
Loſs in ſcowering - - - - -	8
Amount of ſcowered wool - - -	28

It was ſold as ſkin wool for 4*s*. 6*d*. a pound, and of courſe produced 5*l*. 19*s*. or 10*s*. a ſheep, all expences deducted. The amount of this profit was quite unexpected, and holds forth a ſource of advantage in this breed, that has not probably hitherto been calculated upon.

The commendable exertions of Dr. Parry of Bath, in advancing his majeſty's patriotic views as to this object.

Of all who have laboured to render his majeſty's patriotic views in importing Spaniſh ſheep permanently uſeful to his ſubjects, Dr. Parry of Bath deſerves the higheſt commendation. Amidſt the labours of a profeſſion always toilſome when ſucceſsful, and particularly ſo at Bath, where perſons, whoſe diſeaſes cannot be aſcertained by the faculty elſewhere, continually reſort, the doctor found leiſure to employ himſelf in the improvement of the Britiſh fleece, by croſſing various breeds with Spaniſh rams preſented by his majeſty to the Marquis of Bath, and to the Bath Agricultural Society.

The prizes the doctor has continually obtained from the judicious and reſpectable body from whom he borrowed rams, for cloths made of his own wool, in the midſt of a manufacturing country and amongſt abundance of able competitors, prove to a demonſtration, that he has brought the fleeces of the mixed breed very nearly to the value of the original Spaniſh;

niſh; nor is this to be wondered at, when we recollect that the effect of a mixture of breeds operates in the following proportions.

Rate of amelioration of wool by the Spaniſh croſs.

The firſt croſs of a new breed gives to the lamb half of the ram's blood, or 50 *per cent.*

The ſecond gives - - - - - -	75 ditto.
The third - - - - - - -	$87\frac{1}{2}$ ditto.
The fourth - - - - - - -	$93\frac{3}{4}$ ditto.

At which period it is ſaid, that if the ewes have been judiciouſly ſelected, the difference of wool between the original ſtock and the mixed breed is ſcarcely to be diſcerned by the moſt able practitioners.

More need not be ſaid of the doctor's merit. His book, which every man who wiſhes to improve wool ought to read, will give a more juſt idea of the acuteneſs of his diſcrimination, the diligence with which he purſued his purpoſe, and the ſucceſs that finally attended his judicious management, than can be ſtated in the brief form of a report like this.

Lord Somerville's active and judicious labours in eſtabliſhing a Spaniſh flock.

Much, however, as Dr. Parry deſerves the gratitude of all who honour the fleece, Lord Somerville's merit ſtands at leaſt as eminently conſpicuous. Emulating the example of his ſovereign, his lordſhip, whoſe juſt diſcrimination of the value of different breeds of ſtock is admitted by the moſt experienced agriculturiſts, made a voyage to Portugal, for the ſole purpoſe of ſelecting by his own judgment, from the beſt flocks in Spain, ſuch ſheep as joined in the greateſt degree the merit of a good carcaſe, to the ſuperiority in wool which the Merino flocks are allowed to poſſeſs.

His lordſhip ſucceeded, and brought home, more than two years ago, a flock of the firſt quality, which will probably repay with advantage the coſts of the undertaking, as ſome of his lordſhip's rams are ſaid to have been already ſold for 100 guineas each.

His majeſty's flock has proved by ten years experience, that Spaniſh wool does not degenerate in this climate, and that the mutton is excellent.

As ten crops of wool have now been ſhorn from his majeſty's Spaniſh flock, and not a ſingle ſheep from Spain has been introduced into it during the whole of the ten years that have produced them, and as the tenth crop afforded nearly five-ſixths of prime wool and only one-fourteenth of fribbs, it is to be hoped that the deep-rooted prejudice which has for ages deceived the people of England into an opinion that Spaniſh wool degenerates in this climate, will now be finally lodged in that

catalogue

catalogue of vulgar errors, which the increafe of human knowledge daily enlarges. It is to be hoped alfo, that a bold affertion hazarded here, that the mutton of Spanifh fine-wooled fheep is coarfe, tough, and little better than carrion, will be contradicted by the evidence of Mr. Giblet and his cuftomers, to the fatisfaction of thofe who have unwarily given credit to it.

His majefty having been pleafed to permit the fale of fuch fheep as can be fpared from the Spanifh flock to be continued, the rams will be delivered at Windfor, and the ewes at Oatlands, in the latter end of Auguft. As, however, it has been fuggefted to his majefty that the carcafes of the fheep are evidently improved, and that the wool has rather gained than loft in value, fix guineas will be in future the price of a ram, and two that of an ewe. And as his majefty has been gracioufly pleafed to continue to entruft the management of the flock to Sir Jofeph Banks, all letters on the fubject of it, addreffed to him in Soho Square, will be anfwered, and the utmoft endeavours ufed to confult the convenience of thofe who wifh to become purchafers.

July, 1802. JOS. BANKS.

II.

Defcription of a New Procefs of Refining. *By* CIT. DARCET, *Nephew* *.

Caufes of perfection in the arts.

THOUGH the practice of an art is alone fufficient to bring it to perfection, it muft alfo be admitted, that the greater number of ufes to which its products are applied, as well as the greater number of manufactories wherein its proceffes are carried on, are the principal caufes of its arriving quickly at that defirable point; in fact, if the manufactured articles be of general utility, if the proceffes be generally followed, a competition is produced, and every manufacturer endeavours to be victorious in the conteft in which he is engaged: it is then that individual intereft fimplifies the operations of art and brings them to perfection, it fucceeds in affording at a lower price products of a fimilar kind, or in many cafes of a fort preferable

* From the Journal de Phyfique, Vendemaire, An. XI.

to

to thofe obtained by the original proceffes, and it is thus that we daily fee a multitude of the arts arrive at a great degree of perfection. I might produce as examples, the art of the pin-makers, that of the needle-maker, &c. &c. but I mean at prefent to confine myfelf to a new procefs for refining *, the details of which will fufficiently illuftrate what I advance.

Inftances.

It is known in commerce, that articles of gold and filver, after long ufe upon a change of fafhion, or from other circumftances, are melted together without attending to any rule or proportion but that which arifes from the variable courfe of trade.

Standard of melted plate uncertain.

The ingots obtained in thefe operations muft, therefore, at firft, contain different proportions of gold and filver, but they muft at laft be united with thofe metals, which in conformity to the law will require to be added, in order to their employment in the arts of the jeweller and the filverfmith.

To bring thefe metallic compounds into circulation again, that is to fay, to qualify them to be reconverted into works of gold or filver, they muft undergo an operation which reduces them to the ftandard prefcribed by law, there are two methods of accomplifhing this end.

Method of bringing metals to a legal ftandard.

The firft and moft natural is that which points out the addition of the quantity of gold, filver, or copper, neceffary to reftore the fixed proportions of the alloy.

In the fecond all the metals are feparated from each other, and after reducing them to the ftate of pure or fimple bodies, the alloy is formed according to the legal proportions, but the firft of thefe proceffes conftitutes part of the fecond, inafmuch as it employs the fame materials as are purified by means of this laft, which is ufed to feparate an alloy of gold, filver, &c.

It is to this operation that the name of refining is given.

All the operations of refining are founded upon the peculiar properties of thofe bodies on which the refiner is to work. The chief procefs is that which bears the name of *parting*; its bafis is the infolubility of gold in the nitric acid; this acid, by diffolving the filver and copper, leaves the gold, which was alloyed with thefe metals, at the bottom of the veffel in which the folution is carried on.

Refining properly fo called.

Parting.

* I here fpeak of refining as an art, without reference to its connection with the financial operations of the ftate.

This

This is feen by a fingle experiment; but practice foon fhows that for this folution to take place, it muft be made upon an alloy of one part gold with four parts filver, and that it will be more perfect the greater quantity of concentrated acid is employed, or the higher the degree of heat which is applied to the veffels. Thefe obfervations improve the art, but do not bring it to that point which will enable us to obtain the gold in its greateft purity, for this metal very rarely comes from the crucible more pure than 998 or 999 in a thoufand.

Gold not completely purified by the ufual proceffes of parting.

In this ftate it was that I found the art at my entrance into the national refinery, where being placed as it were in competition with the refiners of commerce, I fought the means of improving the proceffes already known, and I now offer to the arts, that which has conftantly fucceeded with me during two years operations carried on in the laboratory of Cit. Dizé.

Antient proceffes.

I fhall begin with defcribing the old proceffes of refining, and afterwards explain the method which I have fubftituted in their ftead.

The ingots received in commerce by the refiners are more or lefs rich, and more or lefs mixed with the fine metals, but their purity is generally between 850 and 950 thoufandth parts. They mix thefe ingots and unite them by fufion, fo that the alloy intended to be formed fhall contain four parts of filver to one of gold; they pour thefe alloys out in grains, and add to every five kilogrammes 500 grammes, to the amount of 650 grammes of nitrate of potafh of the fecond boiling *.

Quartation.

Fufion with nitre.

Granulation.

The mixture is then melted in crucibles, where it is fuffered to cool in a mafs, and thefe maffes are again fufed and granulated, to multiply the furfaces of the alloy to be fubmitted to the action of the nitric acid which is employed in the parting.

Parting by nitric acid.

To perform this operation, the refiner diftributes the grains in pots of ftone-ware, and pours two parts of nitric acid of 30 degrees over one part of the alloy. He places thefe pots in a fand-bath, in order to affift the action of nitric acid upon the filver, by means of the heat. When the folution is ef-

* The intention of this operation is to oxidate the copper and the other metals which are mixed with the gold and filver; and in fact, by this fufion, which is called *(pouffée)* the purity of the mafs is brought to about .978: hence the quantity of acid neceffary for its folution is much diminifhed.

fected,

fected, he decants the nitrate of filver, and wafhes the gold until the water from the wafhings will no longer decompofe muriate of foda.

The gold is not yet freed from all alloy, he therefore adds to it fome nitric acid of the fame ftrength, and brings the mixture to ebullition, he then decants the nitrate of filver, wafhes the gold, and adding to it nitric acid of 40°, he replaces the veffel on the fand-bath. When it has been fufficiently boiled, he feparates the gold from the acid, and wafhes it with great care, dries it by a gentle heat, melts it and forms it into ingots, the purity of which, as I have faid above, does not generally exceed .998, and is even often below .995, which is the limit fixed by law. In the latter cafe, the refiner is obliged to proceed to a new quartation and a new parting; his expences are then doubled and may be even trebled, if in the fecond operation, he does not furmount the inconveniencies which caufed the firft to fail.

Improved procefs.

This is the department of refining which I have brought neareft to perfection, as will be feen in the defcription of my procefs.

Quartation and fufion with only one hundredth part of nitre.

I mix, as in the old operation, the ingots of commerce, in fuch a manner, that when they are melted together, the gold in the alloy fhall bear proportion of one to four parts of filver. I melt the ingots thus prepared, and when the matter is very liquid, I project thereon about 200 grammes of nitrate of potafh for every 20 kilogrammes of the alloy. This fmall quantity of faltpetre is fufficient, as I have remarked, to oxidate the tin, which is always combined in greater or lefs quantity with the ingots of commerce. It is effential to feparate this tin, becaufe in the operation of parting it becomes oxidated and mixed with the gold, and at the time of that metal being melted, it may perhaps be partly reduced and render it harfh and of lefs purity.

Granulation.

When the whole is in perfect fufion, I immediately granulate and diftribute it, as in the former method, into pots. There I add to it the fame quantity of nitrid acid of the fame ftrength, following exactly the fame manipulations as thofe I have defcribed above: in one word I obferve the antient procefs to the moment, when the gold, after having been well wafhed, is put into the crucible: I collect the gold in this ftate into as fmall a veffel as poffible, and add to it as much fulphuric

Parting as before.

The parted gold is boiled with fulphuric acid;

ric of 66° as will cover its ſurface ; I increaſe the temperature of this acid by placing my veſſel on a ſand-bath, I raiſe it to ebullition, and keep it in that ſtate for about an hour ; I then ſuffer the whole to cool *, I decant the acid and waſh the gold until the water from the waſhings will not yield any more precipitate with muriatic acid or alkalies. Nothing then remains but to dry this gold, which is in powder, and to reduce it into ingots, which will conſtantly be of the purity of 1.000, or 24 carats.

and waſhed.

It is then pure.

Theſe are all the details neceſſary to direct the performance of the operation which I propoſe : the following are the advantages which I believe is actually poſſeſſes.

Advantages of the new proceſs.

I make no *pouſſé,* conſequently I ſave both charcoal and crucibles ; I alſo am enabled to perform the operation without pounding the metal ſo often, and have fewer waſhings and meltings : but the greateſt advantage of this improvement is the conſiderable diminution of the fire left in the ſcoriæ ; for in the former method the great quantity of nitrate of potaſh that was added, promoted the oxidation †, or the extreme diviſion of a conſiderable part of the ſilver, which then remained in the ſcoriæ with the oxide of copper, the potaſh, &c. and this waſte amounted, in each operation, to 2400 grammes on ſilver of the purity of .350 to .400. ; and this ſilver remained, without yielding any profit, frequently for ſeveral years ; that is to ſay, until the time when the refiner had a ſufficient quantity of waſhings to make it worth refining.

I have already aſſerted that I conſtantly obtain the gold in its greateſt purity, or at 1.000. which is known to be a great

* I muſt alſo obſerve that it is eſſential to ſuffer the ſulphuric acid boiled with the gold, to be completely cold before it is decanted, without this precaution, as the ſtone-ware veſſels uſed in France are not capable of withſtanding the great degree of heat acquired by the acid in boiling, there is much hazard of being wounded by the fracture of the veſſels, and the drops of acid which are ſcattered in all directions.

For greater ſafety, the bottom of the veſſel may be covered with ſulphuric acid of 66 degrees, and at the temperature of the atmoſphere.

† I dare not yet affirm that the ſilver is in the ſtate of the ſcoræ of the *pouſſée* ; I wait the reſult of ſeveral experiments which I have undertaken on this ſubject in order to fix my opinion.

advantage

advantage in commerce, but it is of ftill more benefit to the refiner, by his operation being certain; and he no longer runs any rifque, if the calculations of his alloy have been accurately made, of being ruined by unfuccefsful operations, and by the neceffity of repeating two or three times a procefs of which nothing can infure the fuccefs.

III.

Remarks on the Conftruction of the Heavens. By WILLIAM HERSCHEL, *LL. D. F. R. S. From the Philof. Tranfactions,* 1802. *p.* 477 *.

IT has hitherto been the chief employment of the phyfical aftronomer, to fearch for new celeftial objects, whatfoever might be their nature or condition; but our ftock of materials is now fo increafed, that we fhould begin to arrange them more fcientifically. The claffification adopted in my catalogues, is little more than an arrangement of the objects for the convenience of the obferver, and may be compared to the difpofition of the books in a library, where the different fizes of the volumes is often more confidered than their contents. But here, in dividing the different parts of which the fidereal heavens are compofed into proper claffes, I fhall have to examine the nature of the various celeftial objects that have been hitherto examined, in order to arrange them in a manner moft conformable to their conftruction. This will bring on fome extenfive confiderations, which would be too long for the compafs of a fingle paper; I fhall therefore now only give an enumeration of the fpecies that offer themfelves already to our view, and leave a particular examination of the feparate divifions, for fome early future occafions.

Celeftial objects have not hitherto been claffed according to their refpective nature.

Introductory obfervations.

In proceeding from the moft fimple to the more complex arrangements, feveral methods, taken from the known laws of gravitation, will be fuggefted, by which the various fyftems under confideration may be maintained; but here alfo we fhall confine ourfelves to a general review of the fubject, as obfer-

* To this Paper is annexed "a catalogue of 500 new nebulæ, nebulous ftars. planetary nebulæ, and clufters of ftars," for which the reader is referred to the Tranfactions.

vation

vation muſt furniſh us firſt with the neceſſary data, to eſtabliſh the application of any one of theſe methods on a proper foundation.

ENUMERATION OF THE PARTS THAT ENTER INTO THE CONSTRUCTION OF THE HEAVENS.

I. *Of inſulated Stars.*

1. Inſulated ſtars;

In beginning our propoſed enumeration, it might be expected that the ſolar ſyſtem would ſtand foremoſt in the liſt; whereas, by treating of inſulated ſtars, we ſeem, as it were, to overlook one of the great component parts of the univerſe. It will, however, ſoon appear that this very ſyſtem, magnificent as it is, can only rank as a ſingle individual belonging to the ſpecies which we are going to conſider.

of which the ſun is one.

They are too far aſunder to be ſenſibly affected by gravitation.

By calling a ſtar inſulated, I do not mean to denote its being totally unconnected with all other ſtars or ſyſtems; for no one, by the laws of gravitation, can be intirely free from the influence of other celeſtial bodies. But, when ſtars are ſituated at ſuch immenſe diſtances from each other as our ſun, Arcturus, Capella, Lyra, Sirius, Canobus, Markab, Bellatrix, Menkar, Shedir, Algorah, Propus, and numberleſs others probably are, we may then look upon them as ſufficiently out of the reach of mutual attractions, to deſerve the name of inſulated ſtars.

Illuſtration: Sirius and the ſun would require 33 millions of years to fall together;

In order not to take this aſſertion for granted, without ſome examination, let us admit, as is highly probable, that the whole orbit of the earth's annual motion does not ſubtend more than an angle of one ſecond of a degree, when ſeen from Sirius. In conſequence of this, it appears by computation, that our ſun and Sirius, if we ſuppoſe their maſſes to be equal, would not fall together in leſs than 33 millions of years, even though they were not impeded by many contrary attractions of other neighbouring inſulated ſtars; and that, conſequently, with the aſſiſtance of the oppoſite energies exerted by ſuch ſurrounding ſtars, theſe two bodies may remain for millions of ages, in a ſtate almoſt equal to undiſturbed reſt. A ſtar thus ſituated may certainly deſerve to be called inſulated, ſince it does not immediately enter into connection with any neighbouring ſtar; and it is therefore highly probable, that our ſun is one of a great number that are in ſimilar circumſtances. To this may be added, that the ſtars we conſider as inſulated are alſo ſurrounded by a magnificent collection of innumerable ſtars, called the

even if there were no contrary attractions.

the milky-way, which muſt occaſion a very powerful balance of oppoſite attractions, to hold the intermediate ſtars in a ſtate of reſt. For, though our ſun, and all the ſtars we ſee, may truly be ſaid to be in the plane of the milky-way, yet I am now convinced, by a long inſpection and continued examination of it, that the milky-way itſelf conſiſts of ſtars very differently ſcattered from thoſe which are immediately about us. But of this, more will be ſaid on another occaſion.

From the detached ſituation of inſulated ſtars, it appears that they are capable of being the centres of extenſive planetary ſyſtems. Of this we have a convincing proof in our ſun, which, according to our claſſification, is one of theſe ſtars. Now, as we enjoy the advantage of being able to view the ſolar ſyſtem in all its parts, by means of our teleſcopes, and are therefore ſufficiently acquainted with it, there will be no occaſion to enter into a detail of its conſtruction.

Inſulated ſtars may be the centers of planetary ſyſtems.

The queſtion will now ariſe, whether every inſulated ſtar be a ſun like ours, attended with planets, ſatellites, and numerous comets? And here, as nothing appears againſt the ſuppoſition, we may from analogy admit the probability of it. But, were we to extend this argument to other ſidereal conſtructions, or, ſtill farther, to every ſtar of the heavens, as has been done frequently, I ſhould not only heſitate, but even think that, from what will be ſaid of ſtars which enter into complicated ſidereal ſyſtems, the contrary is far more likely to be the caſe; and that, probably, we can only look for ſolar ſyſtems among inſulated ſtars.

Probably they all are ſuch: but the complicated ſtars are probably not.

II. *Of Binary ſidereal Syſtems, or double Stars.*

The next part in the conſtruction of the heavens, that offers itſelf to our conſideration, is the union of two ſtars, that are formed together into one ſyſtem, by the laws of attraction.

2. Double ſtars,

If a certain ſtar ſhould be ſituated at any, perhaps immenſe, diſtance behind another, and but very little deviating from the line in which we ſee the firſt, we ſhould then have the appearance of a double ſtar. But theſe ſtars, being totally unconnected, would not form a binary ſyſtem. If, on the contrary, two ſtars ſhould really be ſituated very near each other, and at the ſame time ſo far inſulated as not to be materially affected by the attractions of neighbouring ſtars, they will then compoſe a ſeparate ſyſtem, and remain united by the bond of their own mutual

or ſtars revolving round a common center.

mutual gravitation towards each other. This ſhould be called a real double ſtar; and any two ſtars that are thus mutually connected, form the binary ſidereal ſyſtem which we are now to conſider.

It is eaſy to prove, from the doctrine of gravitation, that two ſtars may be ſo connected together as to perform circles, or ſimilar ellipſes, round their common centre of gravity. In this caſe, they will always move in directions oppoſite and parallel to each other; and their ſyſtem, if not deſtroyed by ſome foreign cauſe, will remain permanent.

Four caſes of orbits.

Figure 1 (Plate V.) repreſents two equal ſtars *a* and *b*, moving in one common circular orbit round the centre *o*, but in the oppoſite directions of *a t* and *b t*. In Fig. 2. we have a ſimilar connection of the two ſtars *a b*; but, as they are of different magnitudes, or contain unequal quantities of matter, they will move in circular orbits of different dimenſions round their common center of gravity *o*. Fig. 3. repreſents equal, and Fig. 4. unequal ſtars, moving in ſimilar elliptical orbits round a common centre; and, in all theſe caſes, the directions of the tangents *t t*, in the places *a b*, where the ſtars are, will be oppoſite and parallel, as will be more fully explained hereafter.

In theſe caſes the center of gravity is conſiderably remote from the larger ſtar.

Theſe four orbits, ſimple as they are, open an extenſive field for reflection, and, I may add, for calculation. They ſhew, even before we come to more complicated combinations, where the ſame will be confirmed, that there is an eſſential difference between the conſtruction of ſolar and ſidereal ſyſtems. In each ſolar ſyſtem, we have a very ponderous attractive centre, by which all the planets, ſatellites, and comets are governed, and kept in their orbits. Sidereal ſyſtems take a greater ſcope: the ſtars of which they are compoſed move round an empty centre, to which they are nevertheleſs as firmly bound as the planets to their maſſy one. It is however not neceſſary here to enlarge on diſtinctions which will hereafter be ſtrongly ſupported by facts, when cluſters of ſtars come to be conſidered. I ſhall only add, that in the ſubordinate bodies of the ſolar ſyſtem itſelf, we have already inſtances, in miniature, as it may be called, of the principle whereby the laws of attraction are applicable to the ſolution of the moſt complicated phenomena of the heavens, by means of revolutions round empty centres. For, although both the earth and its moon are retained in their

orbits by the ſun, yet their mutual ſubordinate ſyſtem is ſuch, that they perform ſecondary monthly revolutions round a centre without a body placed in it. The ſame indeed, though under very narrow limits, may be ſaid of the ſun and each planet itſelf.

That no inſulated ſtars, of nearly an equal ſize and diſtance, can appear double to us, may be proved thus. Let Arcturus and Lyra be the ſtars: theſe, by the rule of inſulation, which we muſt now ſuppoſe can only take place when their diſtance from each other is not leſs than that of Sirius from us, if very accurately placed, would be ſeen under an angle of 60 degrees from each other, They really are at about 59°. Now, in order to make theſe ſtars appear to us near enough to come under the denomination of a double ſtar of the firſt claſs, we ſhould remove the earth from them at leaſt 41253 times farther than Sirius is from us. But the ſpace-penetrating power of a 7-feet reflector, by which my obſervations on double ſtars have been made, cannot intitle us to ſee ſtars at ſuch an immenſe diſtance; for, even the 40-feet teleſcope, as has been ſhewn *, can only reach ſtars of the 1342d magnitude. It follows, therefore, that theſe ſtars could not remain viſible in a 7-feet reflector, if they were ſo far removed as to make their angular diſtance leſs than about $24\frac{1}{4}$ minutes; nor could even the 40-feet teleſcope, under the ſame circumſtances of removal, ſhew them, unleſs they were to be ſeen at leaſt $2\frac{1}{2}$ minutes aſunder. Moreover, this calculation is made on a ſuppoſition that the ſtars of which a double ſtar is compoſed, might be as ſmall as any that can poſſibly be perceived; but if, on the contrary, they ſhould ſtill appear of a conſiderable ſize, it will then be ſo much the more evident that ſuch ſtars cannot have any great real diſtance, and that, conſequently, inſulated ſtars cannot appear double, if they are ſituated at equal diſtances from us. If, however, their arrangement ſhould be ſuch as has been mentioned before, then, one of them being far behind the other, an apparent double ſtar may certainly be produced; but here the appearance of proximity would be deceptive; and the object ſo circumſtanced could not be claſſed in the liſt of binary ſyſtems. However, as we muſt grant, that in particular ſituations ſtars apparently double may be compoſed of ſuch as are inſulated,

If inſulated ſtars be aſſumed to be as far aſunder as Arcturus and Lyra, they cannot by their remoteneſs from the earth appear double;

becauſe they would become inviſible before they were remote enough.

It is highly *improbable* that the double ſtars ſhould conſiſt of ſtars conſiderably aſunder.

* See Phil. Tranſ. for 1800, Part I. page 83.

infulated, it cannot be improper to confult calculation, in order to fee whether it be likely that the 700 double ftars I have given in two catalogues, as well as many more I have fince collected, fhould be of that kind. Such an inquiry, though not very material to our prefent purpofe, will hereafter be of ufe to us, when we come to confider more complicated fyftems. For, if it can be fhown that the odds are very much againft the cafual production of double ftars, the fame argument will be ftill more forcible, when applied to treble, quadruple, or multiple compofitions.

Computation founded on the improbability that the remoter ftar fhould be as much larger than the mean fize as is requifite;

Let us take ♒ Aquarii, for an inftance of computation. This ftar is admitted, by Flamfteed, De la Caille, Bradley, and Mayer, to be of the 4th magnitude. The two ftars that compofe it being equal in brightnefs, each of them may be fuppofed to fhine with half the light of the whole luftre. This, according to our way of reckoning magnitudes *, would make them $4m \times \sqrt{2} = 5\frac{2}{3}m$; that is, of between the 6th and 5th magnitude each. Now, the light we receive from a ftar being as the fquare of its diameter directly, and as the fquare of its diftance inverfely, if one of the ftars of ♒ Aquarii be farther off than the ftars of between the 6th and 5th magnitude are from us, it muft be fo much larger in diameter, in order to give us an equal quantity of light. Let it be at the diftance of the ftars of the 7th maguitude; then its diameter will be to the diameter of the ftar which is neareft to us as 7 to $5\frac{2}{3}$, and its bulk as 1,885 to 1; which is almoft double that of the neareft ftar. Then, putting the number of ftars we call of between the 6th and 5th magnitude at 450, we fhall have 686 of the 7th magnitude to combine with them, fo that they may make up a double ftar of the firft clafs, that is to fay, that the two ftars may not be more than 5″ afunder. The furface of the globe contains 34036131547 circular fpaces, each of 5″ in diameter; fo that each of the 686 ftars will have 49615357 of thefe circles in which it might be placed; but, of all that number, a fingle one would only be the proper fituation in which it could make up a double ftar with one of the 450 given ftars. But thefe odds, which are above $75\frac{1}{2}$ millions to one againft the compofition of ♒ Aquarii, are extremely increafed by our foregoing

and the much greater improbability that the pofition of the two ftars fhould agree with the phenomena.

The odds are many millions to one.

* The expreffions 2m, 3m, 4m, &c. ftand for ftars at the diftance of 2, 3, 4, &c. times that of Sirius, fuppofed unity.

foregoing calculation of the required ſize of the ſtar, which muſt contain nearly double the maſs allotted to other ſtars of the 7th magnitude; of which, therefore, none but this one can be proper for making up the required double ſtar. If the ſtars of the 8th and 9th magnitudes, of which there will be 896 and 1134, ſhould be taken in, by way of increaſing the chance in favour of the ſuppoſed compoſition of our double ſtar, the advantage intended to be obtained by the addition of numbers, will be completely counteracted by the requiſite uncommon bulk of the ſtar which is to ſerve the purpoſe; for, one of the 8th magnitude, ought to be more than $2\frac{1}{4}$ times bigger than the reſt: and, if the compoſition were made by a ſtar of the 9th magnitude, no leſs than four times the bulk of the other ſtar which is to enter the compoſition of the double ſtar would anſwer the purpoſe of its required brightneſs. Hence therefore it is evident, that caſual ſituations will not account for the multiplied phenomena of double ſtars, and that conſequently their exiſtence muſt be owing to the influence of ſome general law of nature; now, as the mutual gravitation of bodies towards each other is quite ſufficient to account for the union of two ſtars, we are authoriſed to aſcribe ſuch combinations to that principle.

Actual obſervation indicates a revolution of double ſtars round their center.

It will not be neceſſary to inſiſt any further on arguments drawn from calculation, as I ſhall ſoon communicate a ſeries of obſervations made on double ſtars, whereby it will be ſeen, that *many of them have actually changed their ſituation with regard to each other, in a progreſſive courſe, denoting a periodical revolution round each other; and that the motion of ſome of them is direct, while that of others is retrograde.* Should theſe obſervations be found ſufficiently concluſive, we may already have their periodical times near enough to calculate, within a certain degree of approximation, the parallax and mutual diſtance of the ſtars which compoſe theſe ſyſtems, by meaſuring their orbits, which ſubtend a viſible angle.

The ſun is not thus combined; though our ſyſtem is not abſolutely at reſt.

Before we leave the ſubject of binary ſyſtems, I ſhould remark, that it evidently appears, that our ſun does not enter into a combination with any other ſtar, ſo as to form one of theſe ſyſtems with it. This could not take place without our immediately perceiving it; and, though we may have good reaſon to believe that our ſyſtem is not perfectly at reſt, yet the cauſes of its proper motion are more probably to be aſcribed to

 ſome

ſome perturbations ariſing from the proper motion of neighbouring ſtars or ſyſtems, than to be placed to the account of a periodical revolution round ſome imaginary diſtant centre.

III. *Of more complicated ſidereal Syſtems, or treble, quadruple, quintuple, and multiple Stars.*

3. Complicated ſidereal ſyſtems: or treble, quadruple, &c. ſtars.

Thoſe who have admitted our arguments for the exiſtence of real double ſtars, will eaſily advance a ſtep farther, and allow that three ſtars may be connected in one mutual ſyſtem of reciprocal attraction. And, as we have from theory pointed out, in figures 1, 2, 3, and 4, how two ſtars may be maintained in a binary ſyſtem, we ſhall here ſhew that three ſtars may likewiſe be preſerved in a permanent connection, by revolving in proper orbits about a common centre of motion.

Obſervations and inferences reſpecting the nature of the poſſible revolutions governed by an attractive force, directed to a center.

In all caſes where ſtars are ſuppoſed to move round an empty centre, in equal periodical times, it may be proved that an imaginary attractive force may be ſuppoſed to be lodged in that centre, which increaſes in a direct ratio of the diſtances. For ſince, in different circles, by the law of centripetal forces, the ſquares of the periodical times are as the radii divided by the central attractive forces, it follows, that when theſe periodical times are equal, the forces will be as the radii. Hence we conclude, that in any ſyſtem of bodies, where the attractive forces of all the reſt upon any one of them, when reduced to a direction as coming from the empty centre, can be ſhewn to be in a direct ratio of the diſtance of that body from the centre, the ſyſtem may revolve together without perturbation, and remain permanently connected without a central body.

Hence may be proved, as has been mentioned before, that two ſtars will move round a hypothetical centre of attraction. For let it be ſuppoſed that the empty centre o, in Fig. 1 and 3, is poſſeſſed of an attractive force, increaſing in the direct ratio of the diſtances $oa : ob$. Then, ſince here ao and bo are equal, the hypothetical attractions will be equal, and the bodies will revolve in equal times. That this agrees with the general law of attraction, is proved thus. The real attraction of b upon a is $\frac{b}{ab^2}$; and that of a upon b is $\frac{a}{ab^2}$; and, ſince $b = a$, it will be $\frac{b}{ab^2} : \frac{a}{ab^2} :: ao : bo$; which was required.

In

Obſervations and inferences reſpecting the nature of the poſſible revolutions governed by an attractive force, directed to a center.

In Figures 2 and 4, when the ſtars a and b are unequal, and their diſtances from o alſo unequal, let $o\,a = n$, and $o\,b = m$; and let the maſs of matter in $a = m$, and in $b = n$. Then the attraction of b on $a = \frac{b}{a\,b^2}$, will be to the attraction of a on $b = \frac{a}{a\,b^2}$, as $n : m$; which is again directly as $a\,o : b\,o$.

Figure of orbits, &c.

I proceed now to explain a combination of three bodies, moving round a centre of hypothetical attraction. Fig. 5 contains a ſingle orbit, wherein three equal bodies $a\,b\,c$, placed at equal diſtances, may revolve permanently. For, the real attraction of b on a will be expreſſed by $\frac{a}{a\,b^2}$; but this, reduced to the direction $a\,o$, will be only $\frac{b\,.\,b\,y}{a\,b^3}$; for, the attraction in the direction $b\,a$ is to that in the direction $b\,y$, parallel to $a\,o$, as $\frac{b}{a\,b^2}$ to $\frac{b\,.\,b\,y}{a\,b^3}$. The attraction alſo of c on a is equal to that of b on a; therefore the whole attraction on a, in a direction towards o, will be expreſſed by $\frac{2\,b\,.\,b\,y}{a\,b^3}$. In the ſame manner we prove, that the attraction of a and c on b, in the direction $b\,o$, is $\frac{2\,a\,.\,b\,y}{a\,b^3}$; and that of a and b on c, in the direction $c\,o$, is $\frac{2\,c\,.\,b\,y}{a\,b^3}$. Hence, $a\,b$ and c being equal, the attractions in the directions $a\,o$ $b\,o$ and $c\,o$ will alſo be equal; and, conſequently, in the direct ratio of theſe diſtances. Or rather, the hypothetical attractions being equal, it proves that, in order to revolve permanently, $a\,b$ and c muſt be equal to each other.

Inſtead of moving in one circular orbit, the three ſtars may revolve in three equal ellipſes, round their common centre of gravity, as in Fig. 6. And here we ſhould remark, that this centre of gravity will be ſituated in the common focus o, of the three ellipſes; and that the abſolute attraction towards that focus, will vary in the inverſe ratio of the ſquares of the diſtances of any one of the ſtars from that centre, while the relative attractions remain in the direct ratio of their ſeveral diſtances from the ſame centre. This will be more fully explained, when we come to conſider the motion of four ſtars.

 A very

A ſtraight-lined orbit or oſcillatory motion.

A very ſingular ſtraight-lined orbit, if ſo it may be called, may alſo exiſt in the following manner. If *a* and *b*, Fig. 7, are two large equal ſtars, which are connected together by their mutual gravitation towards each other, and have ſuch projectile motions as would cauſe them to move in a circular orbit about their common center of gravity, then may a third ſmall ſtar *c*, ſituated in a line drawn through *o*, and at rectangles to the plane deſcribed by the ſtars *a b*, fall freely from reſt, with a gradually acquired motion to *o*; then, paſſing through the plane of the orbit of the two ſtars, it will proceed, but with a gradually retarded motion, to a ſecond point of reſt *d*; and, in this manner, the ſtar *c* may continue to oſcillate between *c* and *d*, in a ſtraight line, paſſing from *c*, through the centre *o*, to *d*, and back again to *c*.

In order to ſee the poſſibility and permanency of this connection the better, let *o* be the centre of gravity of the three bodies, when the oſcillating body is at *c*; then, ſuppoſing the bodies *a* and *b* to be at that moment in the plane *p l*, and admitting *m* to repreſent a body equal in maſs to the two bodies *a b*, *o* will be the common centre of gravity of *m* and *c*. Then, by the force of attraction, the body *c* and the fictitious body *m* will meet in *o*; that is to ſay, the plane *p l*, of the bodies *a b*, will now be at $p' \, l'$. The fictitious body *m* may then be conceived to move on till it comes to *n*, while the body *c* goes to *d*; or, which is the ſame, the plane of the bodies *a b* will now be in the poſition $p'' \, l''$, as much beyond the centre of gravity *o*, as it was on the oppoſite ſide *m*. By this time, both the fictitious body *m*, now at *n*, and the real body *c*, now at *d*, have loſt their motion in oppoſite directions, and begin to approach to their common centre of gravity *o*, in which they will meet a ſecond time. It is evident that the orbit of the two large ſtars will ſuffer conſiderable perturbations, not only in its plane, but alſo in its curvature, which will not remain ſtrictly circular; the conſtruction of the ſyſtem, however, is ſuch as to contain a ſufficient compenſation for every diſturbing force, and will conſequently be in its nature permanent.

In order to add an oſcillating ſtar, it is not neceſſary that the two large ſtars ſhould be ſo ſituated as to move in a circular orbit, without the oſcillating ſtar. In Fig. 8, the ſtars *a* and *b* may have ſuch projectile forces given them as would cauſe them to deſcribe equal ellipſes, of any degree of excentricity.

If

If now the ſmall ſtar *c* be added, the perturbations will undoubtedly affect not only the plane of the orbits of the ſtars, but alſo their figures, which will become irregular moveable ovals. The extent alſo of the oſcillations of the ſtar *c* will be affected; and will ſometimes exceed the limits *c d*, and ſometimes fall ſhort of them. All theſe varieties may eaſily be deduced from what has been already ſaid, when Fig. 7 was conſidered. It is however very evident, that this ſyſtem alſo muſt be permanent; ſince not only the centre of gravity *o* will always be at reſt, but *a o*, whatever may be the perturbations ariſing from the ſituation of *c*, will ſtill remain equal to *b o*.

Obſervations and inferences reſpecting the nature of the poſſible revolutions governed by an attractive force, directed to a center.

It ſhould be remarked, that the vibratory motion of the ſtar *c* will differ much from a cometary orbit, even though the latter ſhould be compreſſed into an evaneſcent ellipſis. For, while the former extends itſelf over the diameter of a globe in which it may be ſuppoſed to be inſcribed, the hypothetical attractive force being ſuppoſed to be placed in its centre, the cometary orbit will only deſcribe a radius of the ſame globe, on account of its requiring a ſolid attractive centre.

After what has been ſaid, it will hardly be neceſſary to add, that with the aſſiſtance of any proper one of the combinations pointed out in the four laſt figures, the appearance of every treble ſtar may be completely explained; eſpecially when the different inclinations of the orbits of the ſtars, to the line of ſight, are taken into conſideration.

If we admit of treble ſtars, we can have no reaſon to oppoſe more complicated connections; and, in order to form an idea how the laws of gravitation may eaſily ſupport ſuch ſyſtems, I have joined ſome additional delineations. A very ſhort explanation of them will be ſufficient.

Fig. 9 (Plate VI.) repreſents four ſtars, *a b c* and *d*, arranged in a line; *a* being equal to *b*, and *c* equal to *d*. Then, if $a\,o = b\,o$, and $c\,o = d\,o$, the centre of gravity will be in *o*; and, with a proper adjuſtment of projectile forces, the four ſtars will revolve in two circular orbits round their common centre. By calculating in the manner already pointed out, it will be found, that when, for inſtance, $a\,o = 1$, $c\,o = 3$, and $c = d = 1$, then the maſs of matter in $a = b$, will be required to be equal to 1,3492.

It is not neceſſary that the projectile force of the four ſtars ſhould be ſuch as will occaſion them to revolve in circles. The

ſyſtem

Obfervations and inferences refpecting the poffible revolutions governed by an attractive force, directed to a center.

fyftem will be equally permanent when they defcribe fimilar ellipfes about the common center of gravity, which will alfo be the common focus of the four ellipfes. In Fig. 10. the ftars $a\ b\ c\ d$, revolving in ellipfes that are fimilar, will always defcribe, at the fame time, equal angles in each ellipfis about the centre of hypothetical attraction; and, when they are removed from $a\ b\ c\ d$ to $a'\ b'\ c'\ d'$, they will ftill be fituated in a ftraight line, and at the fame proportionate diftances from each other as before. By this it appears, as we have already obferved, that the abfolute hypothetical force in the fituation $a'\ b'\ c'\ d'$, compared to what it was when the ftars were at $a\ b\ c\ d$, is inverfely as the fquares of the diftances; but that its comparative exertion on the ftars, in their prefent fituation, is ftill in a direct ratio of their diftances from the centre o, juft as it was when they were at $a\ b\ c\ d$; or, to exprefs the fame perhaps more clearly, the force exerted on a', is to that which was exerted on a as $\frac{1}{\overline{a'o}|^2} : \frac{1}{\overline{ao}|^2}$. But the force exerted on a is to that exerted on c, in our prefent inftance, as $a\,o = 1$ to $c\,o = 3$; and ftill remains in the fame ratio when the ftars are at a' and c'; for the exertion will here be likewife as $a'\,o = 1$ to $c'\,o = 3$.

Fig. 11 reprefents four ftars in one circular orbit; and its calculation is fo fimple, that, after what has been faid of Fig. 5, I need only remark that the ftars may be of any fize, provided their maffes of matter are equal to each other.

It is alfo evident, that the projectile motion of four equal ftars is not confined to that particular adjuftment which will make them revolve in a circle. It will be fufficient, in order to produce a permanent fyftem, if the ftars $a\ b\ c\ d$, in Fig. 12, are impreffed with fuch projectile forces as will make them defcribe equal ellipfes round the common centre o. And, as the fame method of calculation which has been explained with Figs. 6 and 10 may here be ufed, it will not be neceffary to enter into particulars.

Fig. 13 reprefents four ftars, placed fo that, with properly adjufted projectile forces, they may revolve in equal times, and in two different circles, round their common centre of gravity o. If $a\,o = b\,o = 4$, $c\,o = d\,o = 5$, and $c = d = 1$, then will the mafs of matter in $a = b$, required for the purpofe, be

1,5136.

1,5136. This arrangement, remarkable as it may appear, cannot be made in all ſituations; for inſtance, if the diſtance $ao = bo$ were aſſumed equal to 1, that of $co = do$ being 2, it would be impoſſible to find ſuch quantities of matter in a and b as would unite the four ſtars into one ſyſtem.

Obſervations and inferences reſpecting the nature of the poſſible revolutions governed by an attractive force, directed to a center.

As we have ſhewn how the arrangement in Fig. 10. may be derived from that of Fig. 9, ſo it will equally appear, that four ſtars may revolve in different but ſimilar ellipſes round their common centre, as in Fig. 14. For here the four ſtars, when placed at *a b c d*, are exactly in the ſituation repreſented in Fig. 13; but, on account of different projectile forces, they revolve, not as before in concentric circles, but in ſimilar elliptical orbits.

Fig. 15 repreſents three ſtars, *a b c*, in the ſituation of Fig. 5, to which a ſmall oſcillating ſtar, *d*, is added. The addition of ſuch a ſtar to Fig. 1, has been ſufficiently explained in Fig. 7; and, what has been remarked there, may eaſily be applied to our preſent figure. As the fictitious body *m*, in Fig. 7, was made to repreſent the ſtars *a* and *b*, it will now ſtand for the three ſtars *a b* and *c*. If we ſuppoſe theſe ſtars to be of an equal magnitude in both figures, the centre of gravity *o*, of the three ſtars, will not be ſo far from *m* and *n* as in Fig. 7; and the perturbations will be proportionally leſſened.

Fig. 16 gives the ſituation of three ſtars, *a b c*, moving in equal elliptical orbits about their common focus *o*, while the ſtar *d* performs oſcillations between *d* and *e*. What has been ſaid in explaining Fig. 8, will be ſufficient to ſhew, that the preſent arrangement is equally to be admitted among the conſtructions of ſidereal ſyſtems that may be permanent.

We have before remarked, that any appearance of treble ſtars might be explained, by admitting the combinations pointed out in Figs. 5, 6, 7, and 8; and it muſt be equally obvious, that quadruple ſyſtems, under what ſhape ſoever they may ſhow themſelves, whether in ſtraight lines, ſquares, trapezia, or any other ſeemingly the moſt irregular configurations, will readily find a ſolution from one or other of the arrangements of the eight laſt figures.

More numerous combinations of ſtars may ſtill take place, by admitting ſimple and regular perturbations; for then all ſorts of erratic orbits of multiple flexures may have a permanent exiſtence. But, as it would lead me too far, to apply calculation to them, I forbear entering upon the ſubject at preſent.

Before

The numerous combinations actually ſeen in the heavens prove that the preceding diſquiſitions is not mere ſurmiſe.

Before I proceed, it will be proper to remark, that it may poſſibly occur to many, who are not much acquainted with the arrangement of the numberleſs ſtars of the heavens, that what has been ſaid may all be mere uſeleſs ſurmiſe; and that, poſſibly, there may not be the leaſt occaſion for any ſuch ſpeculations upon the ſubject. To this, however, it may be anſwered, that ſuch combinations as I have mentioned, are not the inventions of fancy: they have an actual exiſtence; and, were it neceſſary, I could point them out by thouſands. There is not a ſingle night when, in paſſing over the zones of the heavens by ſweeping, I do not meet with numerous collections of double, treble, quadruple, quintuple, and multiple ſtars, apparently inſulated from other groups, and probably joined in ſome ſmall ſidereal ſyſtem of their own. I do not imagine that I have pointed out the actual manner in which they are held together; but it will always be a deſirable ſtep towards information, if the poſſibility of ſuch unions, in many different ways, can be laid before us; and, very probably, thoſe who have more leiſure to conſider the different combinations of central forces, than a practical aſtronomer can have, may eaſily enlarge on what has been laid down in the foregoing paragraphs.

IV. *Of cluſtering Stars, and the Milky-way.*

4. Cluſtering ſtars and the milky way.

From quadruple, quintuple, and multiple ſtars, we are naturally led to a conſideration of the vaſt collections of ſmall ſtars that are profuſely ſcattered over the milky-way. On a very ſlight examination, it will appear that this immenſe ſtarry aggregation is by no means uniform. The ſtars of which it is compoſed are very unequally ſcattered, and ſhow evident marks of cluſtering together into many ſeparate allotments. By referring to ſome one of theſe cluſtering collections in the heavens, what will be ſaid of them will be much better underſtood, than if we were to treat of them merely in a general way. Let us take the ſpace between β and γ Cygni for an example, in which the ſtars are cluſtering with a kind of diviſion between them, ſo that we may ſuppoſe them to be cluſtering towards two different regions. By a computation, founded on obſervations which aſcertain the number of ſtars in different fields of view, it appears that our ſpace between β and γ, taking an average breadth of about five degrees of it, contains more than 331 thouſand ſtars; and, admitting them to be cluſtering

two

two different ways, we have 165 thoufand for each cluftering collection. Now, as a more particular account of the milky-way will be the fubject of a feparate paper, I fhall only obferve, that the above-mentioned milky appearances deferve the name of cluftering collections, as they are certainly brighter about the middle, and fainter near their undefined borders. For, in my fweeps of the heavens, it has been fully afcertained, that the brightnefs of the milky-way arifes only from ftars; and that their compreffion increafes in proportion to the brightnefs of the milky-way.

We may indeed partly afcribe the increafe, both of brightnefs and of apparent compreffion, to a greater depth of the fpace which contains thefe ftars; but this will equally tend to fhew their cluftering condition: for, fince the increafe of brightnefs is gradual, the fpace containing the cluftering ftars muft tend to a fpherical form, if the gradual increafe of brightnefs is to be explained by the fituation of the ftars.

V. *Of Groups of Stars.*

From cluftering ftars there is but a fhort tranfition to groups of ftars; they are, however, fufficiently diftinct to deferve a feparate notice. A group is a collection of clofely, and almoft equally compreffed ftars, of any figure or outline; it contains no particular condenfation that might point out the feat of an hypothetical central force; and is fufficiently feparated from neighbouring ftars to fhew that it makes a peculiar fyftem of its own. It muft be remembered, that its being a feparate fyftem does not exclude it from the action or influence of other fyftems. We are to underftand this with the fame referve that has been pointed out, when we explained what we called infulated ftars. 5. Groups of ftars.

The conftruction of groups of ftars is perhaps, of all the objects in the heavens, the moft difficult to explain; much lefs can we now enter into a detail of the numerous obfervations I have already made upon this fubject. I therefore proceed in my enumeration.

VI. *Of Clufters of Stars.*

Thefe are certainly the moft magnificent objects that can be feen in the heavens. They are totally different from mere groups of ftars, in their beautiful and artificial arrangement: 6. Clufters of ftars.

their

their form is generally round; and the compreſſion of the ſtars ſhews a gradual, and pretty ſudden accumulation towards the centre, where, aided by the depth of the cluſter, which we can have no doubt is of a globular form, the condenſation is ſuch, that the ſtars are ſufficiently compreſſed to produce a mottled luſtre, nearly amounting to the ſemblance of a nucleus. A centre of attraction is ſo ſtrongly indicated, by all the circumſtances of the appearance of the cluſter, that we cannot doubt a ſingle moment of its exiſtence, either in a ſtate of real ſolidity, or in that of an empty centre, poſſeſſed of an hypothetical force, ariſing from the joint exertion of the numerous ſtars that enter into the compoſition of the cluſter.

The number of obſervations I have to give relating to this article, in which my teleſcopes, eſpecially thoſe of high ſpace-penetrating power, have been of the greateſt ſervice, of courſe can find no room in this enumeration.

VII. *Of Nebulæ.*

7. Nebulæ. Theſe curious objects, which, on account of their great diſtance, can only be ſeen by inſtruments of great ſpace-penetrating power, are perhaps all to be reſolved into the three laſt mentioned ſpecies. Cluſtering collections of ſtars, for inſtance, may eaſily be ſuppoſed ſufficiently removed to preſent us with the appearance of a nebula of any ſhape, which, like the real object of which it is the miniature, will ſeem to be gradually brighter in the middle. Groups of ſtars alſo may, by diſtance, aſſume the ſemblance of nebulous patches; and real cluſters of ſtars, for the ſame reaſon, when their compoſition is beyond the reach of our moſt powerful inſtruments to reſolve them, will appear like round nebulæ that are gradually much brighter in the middle. On this occaſion I muſt remark, that with inſtruments of high ſpace-penetrating powers, ſuch as my 40-feet teleſcope, nebulæ are the objects that may be perceived at the greateſt diſtance. Cluſtering collections of ſtars, much leſs than thoſe we have mentioned before, may eaſily contain 50,000 of them; and, as that number has been choſen for an inſtance of calculating the diſtance at which one of the moſt remote objects might be ſtill viſible*, I ſhall take notice of an evident conſequence attending the reſult of the compu-

* See Phil. Tranſ. for 1800, page 83. N. B In the ſame page, line 22, for 5000 read 50,000.

tation; which is, that a telefcope with a power of penetrating into fpace, like my 40-feet one, has alfo, as it may be called, a power of penetrating into time paft. To explain this, we muft confider that, from the known velocity of light, it may be proved, that when we look at Sirius, the rays which enter the eye cannot have been lefs than fix years and 4½ months coming from that ftar to the obferver. Hence it follows, that when we fee an object of the calculated diftance at which one of thefe very remote nebulæ may ftill be perceived, the rays of light which convey its image to the eye, muft have been more than nineteen hundred and ten thoufand, that is, almoft two millions of years on their way; and that, confequently, fo many years ago, this object muft already have had an exiftence in the fidereal heavens, in order to fend out thofe rays by which we now perceive it.

Immenfe magnitude of the heavens, or fpace containing ftars.

Light employing fix years in its paffage from Sirius; and near two millions of years in arriving from remote nebulæ, thofe objects muft have exifted fo long ago.

VIII. *Of Stars with Burs, or Steller Nebulæ.*

Situated as we are, at an immenfe diftance from the remote parts of the heavens, it is not in the power of telefcopes to refolve many phenomena we can but juft perceive, which, could we have a nearer view of them, might probably fhew themfelves as objects that have long been known to us. A ftellar nebula, perhaps, may be a real clufter of ftars, the whole light of which is gathered fo nearly into one point, as to leave but juft enough of the light of the clufter vifible to produce the appearance of burs. This, however, admits of a doubt.

8. Stellar nebulæ.

IX. *Of milky Nebulofity.*

The phenomenon of milky nebulofity is certainly of a moft interefting nature: it is probably of two different kinds; one of them being deceptive, namely, fuch as arifes from widely extended regions of clofely connected cluftering ftars, contiguous to each other, like the collections that conftruct our milky-way. The other, on the contrary, being real, and poffibly at no very great diftance from us. The changes I have obferved in the great milky nebulofity of Orion, 23 years ago, and which have alfo been noticed by other aftronomers, cannot permit us to look upon this phenomenon as arifing from immenfely diftant regions of fixed ftars. Even Huygens, the difcoverer of it, was already of opinion that, in viewing it, we

9. Milky nebulofity.

faw,

ſaw, as it were, through an opening into a region of light *. Much more would he be convinced now, when changes in its ſhape and luſtre have been ſeen, that its light is not, like that of the milky-way, compoſed of ſtars. To attempt even a gueſs at what this light may be, would be preſumptuous. If it ſhould be ſurmiſed, for inſtance, that this nebuloſity is of the nature of the zodiacal light, we ſhould then be obliged to admit the exiſtence of an effect without its cauſe. An idea of its phoſphorical condition, is not more philoſophical, unleſs we could ſhew from what ſource of phoſphorical matter, ſuch immeaſurable tracts of luminous phenomena could draw their exiſtence, and permanency; for, though minute changes have been obſerved, yet a general reſemblance, allowing for the difference of teleſcopes, is ſtill to be perceived in the great nebuloſity of Orion, even ſince the time of its firſt diſcovery.

X. *Of Nebulous Stars.*

10. Nebulous ſtars.

The nature of theſe remarkable objects is enveloped in much obſcurity. It will probably require ages of obſervations, before we can be enabled to form a proper eſtimate of their condition. That ſtars ſhould have viſible atmoſpheres, of ſuch an extent as thoſe of which I have given the ſituation in this and my former catalogues, is truly ſurpriſing, unleſs we attribute to ſuch atmoſpheres, the quality of ſelf-luminous milky nebuloſity. We can have no reaſon to doubt of the ſtarry nature of the central point; for, in no reſpect whatever does its appearance differ from that of a ſtar of an equal magnitude; but, when the great diſtance of ſuch ſtars is taken into conſideration, the real extent of the ſurrounding nebuloſity is truly wonderful. A very curious one of this kind will be found in the 4th claſs, No. 69.

XI. *Planetary Nebulæ.*

11. Planetary nebulæ.

This ſeems to be a ſpecies of bodies that demands a particular attention. To inveſtigate the planetary nature of theſe nebulæ, is not an eaſy undertaking. If we admit them to contain a great maſs of matter, ſuch as that of which our ſun is compoſed, and that they are, like the ſun, ſurrounded by denſe luminous clouds, it appears evidently that the intrinſic brightneſs of theſe clouds muſt be far inferior to thoſe of the ſun. A

* See *Syſtema Saturnium*, page 8 and 9.

part

part of the ſun's diſk, equal to a circle of 15″ in diameter, would far exceed the greateſt luſtre of the full moon; whereas, the light of a planetary nebula, of an equal ſize, is hardly equal to that of a ſtar of the 8th or 9th magnitude. If, on the other hand, we ſhould ſuppoſe them to be groups, or cluſters of ſtars, at a diſtance ſufficiently great to reduce them to ſo ſmall an apparent diameter, we ſhall be at a loſs to account for their uniform light, if cluſters; or for their circular forms, if mere groups of ſtars.

Perhaps they may be rather allied to nebulous ſtars. For, ſhould the planetary nebulæ with lucid centres, of which the next article will give an account, be an intermediate ſtep between planetary nebulæ and nebulous ſtars, the appearances of theſe different ſpecies, when all the individuals of them are fully examined, might throw a conſiderable light upon the ſubject.

XII. *Of planetary Nebulæ with Centres.*

In my ſecond catalogue of nebula, a ſingle inſtance of a planetary nebula with a bright central point was mentioned; and, in No. 73 of the 4th claſs, is another of very nearly the ſame diameter, which has alſo a lucid, though not quite ſo regular a centre. From ſeveral particularities obſerved in their conſtruction, it would ſeem as if they were related to nebulous ſtars. If we might ſuppoſe that a gradual condenſation of the nebuloſity about a nebulous ſtar could take place, this would be one of them, in a very advanced ſtate of compreſſion. A further diſcuſſion of this point, however, muſt be reſerved to a future opportunity.

12. Planetary nebulæ with centres.

IV.

Deſcription of a New Statical Lamp, which of itſelf raiſes and keeps the Oil at a conſtant Height. By M. D'EDELCRANTZ, *of Stockholm, Member of ſeveral Academies and Literary Societies, Knight of the Order of the Polar Star, &c* *.

SINCE the firſt invention of the lamp with a double current of air by M. Argand, many attempts have been made to find the means of placing the reſervoir of oil below the ſocket of the

Advantages of placing the reſervoir of a lamp below the flame.

* Communicated by the Author.

the lamp, and to raiſe it, in order not only to prevent the diſagreeable ſhadow of lamps on the ordinary conſtruction, which always produces darkneſs in ſome part of the apartment, but alſo to alter the conſtruction, and afford the manufacturing artiſt an opportunity of giving it thoſe elegant forms of which the inſtrument is capable.

A new lamp different from thoſe of Kier and of Carcel.

I ſhall not ſpeak of * Keir's hydroſtatic lamp, nor of that with wheel-work by † Carcel, as they are both well known, but confine myſelf to the deſcription of this new lamp, and which I have conſidered with the ſame deſign as theſe philoſophers.

Deſcription with reference to the drawing.

Deſcription of a new ſtatical lamp.

The body of the lamp, Fig. 1, conſiſts of three veſſels, *aahh*, *dehh*, and *bbfg*. Theſe may be either cylindrical or rectangular, and made of ſheet iron, they differ little in height, and about a line in their diameters, the veſſels *aahh*, and *dehh*, are joined at their lower edges *hh*;—*dehh* is cloſed at *de*; and *aahh* is terminated at *aa*, by a ſmall gallery or baluſtrade by way of ornament. The third veſſel *bbfg*, which paſſes eaſily between the other two, is alſo cloſed at *fg*, by a plate which projects about three lines; from the center of this there riſes a tube *kkll*, to the extremity *ll* of which is ſcrewed the ſocket of a common Argand's lamp. In the center of this is fixed another tube *pq*, made of tin, in which the iron wire *mn* is placed, and is beſides fixed to the plate *de*, at right angles to its ſurface, the end of the wire is furniſhed with a nut and ſcrew *oo*. This wire ſcrews to direct the veſſel *bbfg* in its motion between the two others, and the poſition of the nut is intended to determine the limits of that motion.

To uſe this lamp, let us ſuppoſe the nut *oo* to be ſo fixed, that when the veſſel *bbfg* is at its greateſt height, there ſhall only remain a ſpace of 14 or 15 lines between the two other veſſels, that is to ſay, a length equal to *db*. Now pour mercury into the ſpace between the two veſſels to within the diſtance of a few lines at *de*, as to *rr*; the edge of the middle veſſel will then be immerſed in mercury, and all communication between the inſide of this veſſel and the outer air will be cut off. And if after having unſcrewed the ſocket of the lamp, oil be poured into the inſtrument by the aperture *ll*, it will

* Philoſ. Journal, Quarto, III. 467.

† Philoſ. Journal, New Series, II. 108.

occupy

occupy the ſpace between the two ends *fg* and *de*, and the oil by preſſing on the ſurface of the mercury at *rr*, will raiſe it a little on the outſide from its height and ſpecific gravity. The ſpecific gravity of oil to that of mercury is about as one to 16, a column of oil therefore of 16 inches would not raiſe the mercury above an inch *. Deſcription of a new ſtatical lamp.

If, after having filled the reſervoir with oil, and ſcrewed on the ſocket, the Plate *fg* be ſo adjuſted with weights, that it ſhall become equal in weight to a column of oil whoſe baſe is *fg*, and height *ks*; it is evident that the oil in the reſervoir will under this preſſure riſe to *ss*, and will conſtantly remain at that height while any remains, and while the weight of *fg* continues unaltered, notwithſtanding the fall of *fg*, which when the oil is conſumed, will be depreſſed to *de*, and reſt on the Plate *de*. *uutt*, is a ſmall tin tube, intended to receive the oil which may be ſpilled; it anſwers the purpoſe of the ſmall round glaſs uſually hung beneath other lamps.

The height to which the oil may be raiſed in a lamp of this kind is arbitrary, ſince to increaſe the height of the ſocket it is only neceſſary to increaſe the weight in proportion, always providing a ſpace for the riſe of the mercury, equal to a 16th part of the riſe of the oil. This riſe will be conſtant, the weight being always the ſame, as it does not in any reſpect depend on the quantity of oil in the reſervoir.

It is neceſſary, however, to obſerve reſpecting the invariability of the counterpoiſing weight, that it will not be geometrically accurate. We know by the laws of hydroſtatics, that a ſolid body loſes by immerſion in any fluid as much of its weight as is equal to the weight of the fluid diſplaced by it; now as the oil is conſumed, the middle veſſel ſinks deeper into the mercury, and conſequently loſes of its weight, it will therefore after ſome time be evidently incapable of ſupporting the column of oil at the ſame height as before. But if this middle veſſel be made of very thin ſheet iron and has not much range, this diminution will be inconſiderable, and may in practice be entirely neglected. The lamp in the figure being adjuſted to 10 lines, will contain oil ſufficient for eight hours, at the end of this time, the ſurface of the oil in the ſocket will

* *i. e.* Half an inch riſe without and half an inch depreſſion within. N.

have

Defcription of a new ftatical lamp.

have fallen but two or three lines, whilft the flame will not be materially injured by a fall of four or five. By admitting a larger interval between *de* and *fg*, the lamp will hold more oil and burn longer. To raife the oil to its proper height on thefe occafions, it will be neceffary to add more weights; for example, one ounce for each line of the height required.

The method of filling and ufing the lamp will naturally be underftood from the foregoing defcription. I fhall only add, that the quantity of mercury required will be about a pound, and that when once put into the lamp it need not be removed from thence, unlefs it fhould be thought proper to clean the infide of the lamp at intervals. In this cafe the oil and mercury may be poured together into a glafs or earthen veffel, and the mercury afterwards feparated from the oil by a paper ftrainer. The inftrument may be cleaned with warm water and a feather, it may then be filled again, and when the proper weights are placed, the rife of the oil muft be produced by a gentle fhaking of the lamp, any unufual preffure for this purpofe muft be avoided. The oil will not rife to its proper and conftant height till a few moments after being lighted. The length of the lower current of air requires high chimneys to produce a clear light. Thefe muft be taken off when the lamp is extinguifhed. To remove it with fafety, the column or upper part of it muft be taken off, and the inftrument be carried carefully by its focket.

It will eafily appear, that various ornamental and elegant forms may be given to this lamp, the determination of which will remain with the manufacturing artift. Our prefent defign reprefents a truncated column, on a fquare bafe, ornamented with four balls, which alfo ferve for the counterpoifing weights.

I have thought proper to call this inftrument a ftatical lamp, as depending on the equilibrium of three different bodies, two of them fluid, and one folid.

Ou

V.

On Oily Hidrogen. By Professor PROUST *.

THE perusal of the memoir of the Dutch chemists having suggested to me the idea of making two experiments on the gas obtained by the distillation of olive-oil, I apprehend, doctor, that you will see the result with pleasure. With respect to the inferences which I have ventured to draw from them, if they be not true, they will nevertheless contribute to extend the scale of this new order of facts, and increase the means of studying them with better effect. Gas by distillation of olive oil.

I call this gas oily, because it seems to me that its great weight; its white, footy, heavy flame; its strong smell; and above all, its property of becoming lighter by being passed several times over alcohol, point out a simple solution of the oily vapour in carbonated hidrogen. Called oily gas;

The purest oil must yield a considerable quantity of carbonic acid with the oily gas. Berthollet was perfectly right in saying that a certain dose of oxigen would be found in the oils: besides, the absorption which they continually make authorises it. It may perhaps be attempted to connect this acid with the principles of that mucous body which Scheele separated from them by oxide of lead; but if on the one hand, the nature of the ingredients in this operation be well considered, and on the other, that of the sugar of oils, I believe it will not be unreasonable to suppose that the sugar was entirely formed during their coction with the oxide. Pure oils yield carbonic acid with the oily gas: they contain oxigen. Sugar of oils;

It is free from every metallic matter, since it does not render hidro-sulphurated water turbid; but it is particularly distinguishable from all our vegetable sugars, because it is not susceptible of any fermentation, corruption or mouldiness. I have kept three or four ounces of it full ten years, which is of the consistence of a tolerably clear syrup, without its having experienced the slightest alteration in the greatest heats. In a word, I am very much disposed to believe that this species of sugar is rather a new product than a substance separated from the oils. not the same as the vegetable sugars. Its unchangeable nature.

* From the Journal de Physique, Germinal, An. XI.

Diftilled oil again difcovers characters to which little attention has been given. It is changed into a volatile odorous or effential oil, doubtlefs by diffolving a certain quantity of carbonated hidrogen. The following are fome of its properties:

Properties of diftilled oil.

When heated with water in a retort, part of it comes over in the diftillation. It has a very powerful but difagreeable fmell, with a degree of lightnefs which places it in the rank of volatile oils, becaufe its weight to that of olive-oil is not more than as 91 to 100. It fwims upon alcohol, which diffolves it with eafe. Laftly, it enters into ebullition as readily as fpirit of turpentine; and if a lighted taper be brought to the mouth of the matrafs, the vapour of it burns like that of volatile oil.

Sebate of potafh remains after the diftillation of foap.

I have not particularly examined the acid liquor which accompanies the diftilled oil: I fhall only infert a fact relative to its hiftory. If foap be diftilled to the deftruction of its oil, the faline refidue which I fhould have expected to be the carbonate, is found to be the febate of potafh. It is cryftallizable, and the fulphuric acid inftantly difengages that penetrating vapour called febacic acid. But to return to the oily hidrogen, which holds in folution the factitious effential oil which I have mentioned at the beginning. When the heat fuddenly ftrikes that portion of the oil which is found there, as if in a red hot tube, it paffes from the vapourous ftate to that of carbonated hidrogen, in the fame way as happens to ether, alcohol, radical vinegar, effence of turpentine, &c. when they pafs through a red-hot tube.

I fhall conclude this by an experiment which Rouelle made in his lectures, and which I alfo fhow in mine; becaufe in addition to the agreeable fpectacle, the explanation of its caufes is perfectly conformable to the theory of inflammations.

Beautiful appearance from the fudden combuftion of oil;

Pour half a fpoonful of olive-oil into a fmall melting crucible obfcurely red, or at that degree of heat which fhall have been difcovered by fome previous experiments. A thick cloud of white fmoke will immediately rife which takes fire at its fummit, that is to fay, four or five feet diftant from the crucible. If on the contrary, the combuftion of the cloud arifes from the crucible, it is occafioned by the heat being too great. In that cafe we muft wait a few minutes, and the phenomenon will appear in all its magnificence. A piece of wax will give the fame effects, but it feems to me to require more previous preparations than the oil.

and wax.

On

On the Difoxidation of Iron.

The fimpleft truths of chemiftry are not always thofe which are the moft eafily fhown in public teaching. For example, nothing can be more evident to the moft numerous auditory than that, in the tube experiment, the iron carries with it an overcharge of oxigen, becaufe every one of the fpectators can fatisfy himfelf by his eyes, and acquire an abfolute conviction of that which is intended to be fhown; but when amidft the connection of ideas and proofs, we come to thofe which relate to the difoxidation of this metal, we muft quit facts and replace them by fuppofitions, becaufe it is not poffible to execute them with facility in public. In fact, it is impoffible either to apply a degree of heat in a few inftants to the oxide of iron, fufficient to deoxidate it, nor can it even be eafily reduced by melting. But by the following procefs this difficulty is partly overcome.

The fimpleft truths of chemiftry not always eafy to be manifefted in a lecture.

Inftance. Difoxidation of iron

Heat for an hour a mixture of one ounce of iron ore of Elba, and two gros of charcoal, in a fmall coated glafs retort, at the bottom of which the clay has been left of the thicknefs of two inches. Place it immediately on the grate of the furnace, and its neck will then project from the furnace by the door of the fire-place. Clofe it with fragments of bricks and earth, which will keep the retort in its fituation. If the neck be not long enough to reach into the pneumatic-trough, which in this cafe ought to be a fallad-difh or a foup-plate covered with a plate of lead perforated, add to it another which muft be luted with paper and ftarch.

Eafy procefs for this deoxidation by Elba. Iron ore heated in a glafs retort with charcoal.

Having done this, cover with the ufual precautions, the grate and the retort with charcoal. It receives from this difpofition a much more intenfe heat than if it had been placed on the bars. The carbonic acid and carbonated oxide are then obtained with the greateft facility.

When the retort is cold, its belly will be found to be nearly filled with pure iron of a fpongy appearance, which acquires the metallic brilliancy by friction with any polifhed fubftance. This mafs is broken with difficulty, and notwithftanding the feparation of its parts, they will be all found to be foldered together. On heating it dexteroufly by the blow-pipe, it burns with fparkling. The magnet attracts the whole of it. Part of it may be ufed to give hidrogen with fulphuric acid, in

State of the reduced iron.

 fhort

ſhort every one is convinced on touching it, that it is iron reduced to its primitive ſtate, and that if it were formed into a maſs, it might conſtitute bar-iron.

Bituminous odour from its ſolution.

As carburet is formed in this operation, the diſſolution of the iron yields the ſame bituminous ſmell as that which is obſerved in crude iron.

The proceſs with coal of blood,

If inſtead of wood-charcoal, that of blood well waſhed be mixed with the oxide, ammoniacal carbonate will be depoſited in the neck of the retort. I have explained the cauſe of this product in my ſecond memoir on Pruſſian blue. The ſolution of iron then affords phoſphate with the potaſh at the firſt moment of its precipitation. The bituminous odour of the ſolution of caſt iron as well as the gas, are alſo obtained from recently made charcoal of blood, by the application of ſulphuric acid, it takes fire at the mouth of the matraſs. The reaſon of this is ſo evident, that it would be ſuperfluous to dwell on it.

gives ammoniacal gas.

Inflammation.

Four inches of this gas, and one of oxigen, cannot be inflamed in a tube with the charge of a pocket electrophorus; neither can it with two of oxigen, nor even with three. My object in theſe experiments was to diſcover if charcoal or oil would be precipitated.

with oxigen;

Laſtly, with four inches of oxigen or equal parts an inflammation takes place; but it cauſes an agreeable ſurpriſe to obſerve that, on opening the ſtop-cock, the gazeous reſidue inſtead of being diminiſhed, is on the contrary, increaſed to thirteen or fourteen inches. This ſudden expanſion is accompanied by another fact equally curious, the explanation of which is obvious, on recollecting the obſervation of Monge, on an atmoſphere more or leſs compreſſed. This is a cloud or thick miſt which fills the tube as the reſidue eſcapes from its former compreſſion. Let us now examine our eight inches of mixture which has increaſed in bulk to thirteen or fourteen by the inflammation.

attended with expanſion.

On transferring it through lime-water, it will be found that it ſcarcely troubles it, and even at ſometimes not perceptibly at all. It alſo, within a few lines, preſerves its dimenſions of thirteen or fourteen inches.

If a light be preſented to it, it will be ſeen that it is not oily hidrogen. Its blue flame deſcends ſlowly, and it is conſumed without the leaſt detonation. Laſtly, it has but a weak odour very different from that of oily hidrogen.

This

This gas burnt with oxigen is wholly converted into carbonic acid, and is no longer dilatable however ſmall the quantity of oxigen mixed with it may be.

The expanded gas produces carbonic acid when burned with oxigen.

The following are the conjectures which I form at preſent from theſe reſults.

I do not think that the reſidue retains the leaſt trace of oxigen after the inflammation, otherwiſe it would burn with rapidity, which is far from being the caſe.

Conjectures or inferences.

Four inches of oxigen generally conſume ſeven and a half of pure hidrogen in my eudiometer, and as our reſiduary gas contains only a very ſmall quantity of carbonic acid, it follows, that theſe four inches have conſumed a quantity of hidrogen which would have been at leaſt equal to ſeven inches, had it been in the uſual ſtate of dilatation at which the preſſure of the atmoſphere keeps it.

Four inches of oily hidrogen therefore contain, according to this, ſeven inches of pure hidrogen condenſed into the bulk of four.

Again our reſidue after the combuſtion of four inches of oily gas, is all at once changed into thirteen or fourteen inches of carbonated hidrogen. We muſt therefore conclude, that the ſeven inches of hidrogen compreſſed into the bulk of four, held in ſolution a quantity of oil capable of being converted, by a high temperature, into thirteen or fourteen inches of carbonated hidrogen.

I ſay a high temperature, becauſe in fact, that alone is capable of changing oil into carbonated hidrogen.

Whence I conclude, that in the inflammation of four parts of oily gas, by an equal quantity of oxigen, nothing is really burnt but the hidrogen.

VI.

Letter from FORTIS *to* J. C. DELAMETHERIE, *on a Shower of Mud which fell at Udina* *.

I HAVE juſt received at the ſame time, the number of the Journal de Phyſique, in which you notice the different opinions reſpecting ſtones fallen from above, of which almoſt every

* In the Journal de Phyſique, Germinal, An. XI.

country

Muddy ſhower which extended over ſeveral leagues.

country has a tale or hiſtory to relate, and the details by my friend Cernazaï, a good mineralogiſt of Udina, of a ſlimy rain which actually fell there in the evening of the 6th of this month, the wind having blown with violence from the eaſt for ſeveral days. The ſurface of the land which has been entirely watered with this ſtrange rain, appears to be from ten to twelve leagues in diameter from the ſea-ſhore to the foot of the Alps of Carnia. Here, my dear friend are the means of eſtabliſhing a brick furnace in the higher regions of the atmoſphere, by thoſe who are already convinced that materials are ſuſpended there, ſuſceptible of a fuſion ſimilar to that of the interior of a volcano. I know not if the partiſans of that opinion which brings down the lavas from the moon, will be able to derive any ſupport from the mud which has juſt coated the fields of Friuli; but for my own part, I was at firſt of opinion, that the wind having been charged, in Sicily or near Naples, with volcanic duſt thrown up by a whirlwind, had depoſited it at the foot of the mountains of Carnia, which had obſtructed the further progreſs of the clouds. But having ſince examined a ſpecimen of the ſediment in queſtion, ſent to me by my friend at Udina, and which I forward to you with this letter *, by a very powerful magnifier, I am convinced that there is not the leaſt reſemblance between this argillaceous earth and the *detritus* thrown by volcanoes into the upper regions of the atmoſphere. It appears to me much more probable that a hurricane, or perhaps a water-ſpout had pumped up the ſlimy water left by the large rivers when they overflow our richeſt plains, and raiſed it into theſe regions in which the wind had acted upon it. It is in conſequence of ſuch very natural and common events, that worms, tadpoles, and ſmall fiſhes are frequently ſeen to fall from on high, without any one dreaming of their proceeding from an aerial race or from another globe.

Its probable nature.

You, my dear and learned friend, may make what uſe you think proper of this ſimple unſkilful explanation.

* This earth is light, of the colour of brick-duſt, and ſeems to be argillaceous. *Note of J. C. Delametherie.*

VII.

Letter from M. VAN MARUM *to* M. BERTHOLLET. *Containing an Account of ſome Experiments, ſhewing the Method of extinguiſhing violent Fires with very ſmall Quantities of Water, by Means of Portable Pumps.*

SIR,

WHEN I had the pleaſure to ſee you, during my ſtay in Paris, in September laſt, you informed me that my experiments, made on a large ſcale ſome years ago, to ſhew the practicability of extinguiſhing very violent fires with an inconſiderable quantity of water, were entirely unknown in France. You at the ſame time requeſted me to ſend you an account of the experiments to be read at the national inſtitute, and inſerted in the *Annales de Chemie.* My compliance with your requeſt has been prevented till now by the want of time. The circumſtances which gave riſe to the experiments were the following:

History of an experiment for extinguiſhing fire.

Nine years ago, Van Aken, a Swede, publiſhed at Stockholm, Copenhagen, and Berlin, that he could very quickly extinguiſh large artificial fires with an inconſiderable quantity of a liquid which he called anti-incendiary, the compoſition of which he had for ſome time kept ſecret. Having obſerved in the journals, that M. Van Aken had repeated his experiments with much ſucceſs in Berlin, before ſome members of the academy of ſciences, I wrote to the celebrated M. Klaproth, requeſting, if he knew the compoſition of the anti-incendiary liquid, he would communicate it to me; intending to ſhew, at this place, the merit of the invention, by an experiment on a large ſcale. With this intention, as ſoon as I had received M. Klaproth's communication, I cauſed Van Aken's liquid to be prepared under my immediate inſpection. The compoſition was a ſolution of 40lb. of ſulphate of iron, and 30lb. of ſulphate of alumine, mixed with 20lb. of the red oxide of iron, and 200lb. of clay. I then commenced a ſeries of comparative experiments, by forming two combuſtible maſſes, alike in all circumſtances; ſetting them on fire, and extinguiſhing one of them with the liquid of Van Aken, and the other with common water, I was much ſurpriſed to find, in ſeveral trials, that, by

Anti-incendiary water of Van Aken.

Its compoſition.

On trial it proved less effectual than common water.

by uſing the two liquids in the ſame manner, the fire was always extinguiſhed more quickly by the common water than by the anti-incendiary liquor; but I obſerved, at the ſame time, that a very inconſiderable quantity of water, if judiciouſly directed, would extinguiſh a very violent fire. The reſult of my firſt experiments in this reſpect, led me to make others on a larger ſcale, and which I ſhall deſcribe alone: I took two caſks, which had been full of pitch, and of which the inſide was yet well covered with that inflammable ſubſtance; the heads of theſe were then taken out, and in order to aſſiſt the operation of combuſtion, I gave them a conical ſhape, placing the larger aperture of 20 inches diameter uppermoſt, the other of 16 inches diameter below, mounted on a three-legged ſtand, a few inches from the ground, ſo that the fire might be kept as briſk as poſſible by the free current of air paſſing through the caſk. I covered the inſide of each caſk with a freſh coat of pitch, and having placed ſhavings of wood in each caſh, I lighted them one after the other. I began to extinguiſh this fire when it was at its height. For this purpoſe I made uſe of an iron ladle capable of holding two ounces of water, and provided with a very long handle, being kept at the diſtance of four or five feet from the apparatus by the violence of the fire. I carefully poured the water from this ladle in very ſmall ſtreams over the inſide of the caſk, placing the ladle on the edge of the caſk, and moving it gradually along the edge as the flames ceaſed. In this way the firſt ladle full of water extinguiſhed nearly one half of the fire, and what remained was effectually put out by a ſecond ladle full, uſed in the ſame manner. The ſtriking ſucceſs of this experiment, induced me to repeat it in the preſence of many perſons, and with care and attention in the application and management of the water, I have more than once ſucceeded in extinguiſhing an highly ignited caſk with a ſingle ladlefull of two ounces of water.

Experiment to ſhew that a very ſmall quantity of water is needful in extinguiſhing fire.

Theory. The ſteam keeps off the acceſs of air.

It will appear ſurpriſing at firſt that ſo violent a fire could be extinguiſhed with ſo ſmall a quantity of water; the reaſon will, however, be eaſily underſtood when we reflect that, according to well-known facts and principles, the flame of a burning body will ceaſe whenever its contact with the atmoſpheric air is prevented: now, when a ſmall quantity of water is thrown upon an highly-ignited body, part of that water is inſtantly

inſtantly reduced to ſteam, which, riſing from the ſurface of the burning body, prevents the contact of the atmoſpheric air, and by that means puts out the flame, which cannot appear again while the production of the ſteam is continued.

According to theſe experiments, it appears that the art of extinguiſhing a violent fire with a ſmall quantity of water, conſiſts in this: that the water be thrown on that part of the fire which is the moſt violent; ſo that the quantity of ſteam produced, which ſuppreſſes the flame, may be the greateſt poſſible: that water be continued to be thrown on the neigbouring inflamed parts, as ſoon as the fire has ceaſed in that on which the operation was began, and that all the burning parts be viſited in this way as quickly as poſſible. By thus following the flames regularly with ſtreams of water, they may be every where ſuppreſſed before the part on which the operation was began, ſhall have entirely loſt by evaporation the water with which it was moiſtened: this is often neceſſary to prevent the parts from breaking out afreſh; for, on the principle above-mentioned, a burning body, of which the flames are ſuppreſſed, cannot be again in flames until the water thrown on it be totally evaporated.

Inſtructions grounded on the facts and theory.

Being convinced by theſe experiments, that a ſmall quantity of water is ſufficient to extinguiſh ordinary fires, eſpecially in an early ſtage. I have endeavoured to produce the ſame conviction among my fellow-citizens, by repeating the experiments I have deſcribed, and have adviſed them to provide themſelves with ſmall portable pumps for their uſe in caſe of neceſſity. Many immediately followed my advice, and their good effect being proved, the number has gradually increaſed in ſeveral towns in Holland, eſpecially after an experiment which I made in this place in May, 1797, to ſhew, on a larger ſcale, the advantages to be derived from a well-regulated direction of ſtreams of water, in the extinguiſhing even the moſt violent fires, and with very ſmall quantities of water, where portable pumps are uſed.

Experiment on a larger ſcale.

The following is the experiment:

I prepared a ſhell of dry wood, forming a room of 24 feet long, 23 feet wide, and fourteen feet high, having two doors on one ſide, and two windows on the other; this box, provided with the wood-work of a roof, was uncovered above, and its partition raiſed ſix inches from the ground; ſo that, having a current

A wooden edifice ſet on fire and extinguiſhed.

rent of air from the bottom upwards, the combuſtion might be rendered as briſk as poſſible when the building ſhould be ſet on fire. The inſide of the partitions were ſtrongly pitched, and were beſide covered with twiſted ſtraw, which was alſo pitched. To the inner part of this ſtraw covering, I faſtened wood ſhavings, and beſides this a coarſe cotton cloth, ſoaked in oil of turpentine, in order that the whole inſide of the building might be quickly on fire. Very ſoon after lighting it, the flames being rendered more briſk by the wind, were every where ſo violent, that it was conſidered by my aſſiſtants as impoſſible to extinguiſh them: I ſucceeded, however, after the method above directed, in little more than four minutes, and with five buckets of water, a part of which was waſted by the negligence of my aſſiſtants, as was proved by the following experiment:

Repetition: Having invited only a few perſons to witneſs this, my firſt experiment, on the 8th May, I repaired my apparatus, and repeated it on the 11th, before a great number of ſpectators: the flames were no leſs violent than in the preceding experiment. I now undertook to direct the jets of water in perſon, and ſucceeded in extinguiſhing the fire in three minutes, and employed only three buckets of water, each containing about eighteen pints.

Experiment at Gotha. Being at Gotha in July 1801, the duke and ducheſs of Gotha preſſed me much for a repetition of my experiment at their own expence, the details of which they had ſeen in the German journals; in order to make the matter more fully underſtood in that part of Germany, where, as in every other place, fires often make the moſt dreadful ravages, in conſequence of the ignorance of the method of properly uſing the ſmall quantity of water which they may have at hand. The obliging manner in which their highneſſes made their requeſt, and my own wiſh to render it more generally uſeful, induced me to repeat it. The celebrated aſtronomer, Von Zach, aſſiſted me in this experiment, and had the goodneſs to inſert an account of it in a German periodical paper, intitled *Reichs Anrieger*, 6th Auguſt, 1798, No. 119.

M. Lalande arriving at Gotha on the 30th July, four days after this experiment, was well acquainted with its reſults, and, as he has ſince told me, ſpoke of it at the National Inſtitute, ſoon after his return to Paris; but informed me, at the ſame time, that

that the truth of his account was doubted. In order to clear away all doubt on that head, I ſubjoin the following liberal tranſlation of the verbal procefs of the experiment which Von Zach had drawn up, and inſerted in the above-mentioned periodical paper:

Verbal procefs of Von Zach on the method of extinguiſhing fire.

Doctor Van Marum * being at Gotha, in the courſe of a literary journey, which he was making in Germany in 1798, the duke of Gotha, well known as an amateur of the phyſical and mathematical ſciences, ſignified his deſire to ſee, on a large ſcale, that experiment of extinguiſhing fires, the effect of which M. Van Marum had ſhewn him, in extinguiſhing a pitched caſk, ſet on fire, with a ſmall ladle of water. He cauſed a building to be conſtructed under M. Van Marum's direction, in the ducheſs's garden, of equal dimenſions in all reſpects with that uſed for the ſame experiment at Harlaem, which was of 24 feet long, 20 feet wide, and 14 feet high. It had two doors on the north-eaſt ſide, and two window-like openings on the north-weſt ſide; the top was left entirely open to give the flames a free vent; the inſide of this receptacle was plaſtered with pitch, and afterwards covered with ſtraw matts, on which melted pitch had been poured. To the bottom of theſe matts, cotton wicks, ſoaked in ſpirits of turpentine, were ſuſpended, that ſo the building might be every where on fire at the ſame time. In this ſtate the fire, excited by the wind, was ſoon ſo violent, that the flames, with thick clouds of ſmoke, were carried ſeveral feet above the opening of the roof, and ſo fiercely, that the ſpectators aſſembled about the building quickly drew back; many were of opinion that it would not be poſſible to extinguiſh it, but that the building muſt be reduced to aſhes. When the ſtraw mats were entirely conſumed, the interior wood work of the building was ſoon on fire in every part. The moſt unfavourable circumſtances attended this experiment; for the wind drove the flames directly through the two doors on the north-eaſt ſide, by which it had been intended to introduce the ſtreams of water to extinguiſh them: but notwithſtanding this, M. Van Marum placed a ſmall portable pump (or engine) before the door in that part of the north-eaſt ſide of the building neareſt to the ſouth-eaſt-ſide;

* The remainder of this memoir being the words of the report, it has not been marked as a quotation.---N.

without

Verbal procefs of Von Zach, on the method of extinguifhing fire.

without regarding the apprehenfions of his affiftants, he fet it in action, and placing himfelf before this door, as near as the intenfe heat of the fire would admit, he directed the ftreams of water firft towards the fouth-eaft fide, and as near the door as was poffible, and continued until the flames were extinguifhed on the fide, and fprinkled with water in the fame direction; after which the water was directed along the fouth-eaft fide, and afterwards the north-eaft, fo that in a few minutes the flames were got under, and the burning partitions were extinguifhed. After this the pump was placed before one of the openings in the north-weft fide. He alfo very foon extinguifhed the fouth-eaft fide; and laftly, coming to the centre of the building, where the fire appeared here and there in the chinks of the boards and the holes left by the nails, breaking out at intervals in fmall flafhes, he entirely extinguifhed them, and thus perfectly fubdued this violent fire. It was eftimated by feveral of the fpectators, that the fire was extinguifhed at moft in three minutes from the commencement of the action of the pumps to the time when the wood juft remained burning, and broke out afrefh in fome places; thefe renewals were however fo inconfiderable, that the burning parts were quenched by means of fome wet rags faftened to a ftick. Before fetting the pump to work, its refervoir was filled with four buckets of water; but in carrying it to the firft window of the building, and from thence to the middle of it, near a bucket full was fpilt; fo that it may be pofitively affirmed, that this violent fire was extinguifhed with three buckets of water, excluding that which was afterwards ufed to quench thefe parts of the building which remained red. It was eafily obferved, when the fire was out, that not only the ftraw matts were burned, but the entire wood work of the building had been on fire, infomuch that the fmalleft part of wood could not be found in the infide of the building which had not been more or lefs feverely burnt. The north-eaft fide in particular, againft which the wind drove the flames with the moft violence, was entirely charred.

The experiment made at Gotha differed materially from that at Harlaem, as the flames and thick fmoke, which came out at the doors on the latter experiment, rendered accefs to the building with the pump very difficult; fo that it was only by means of perfuafion, and the courageous example which

M. Van

M. Van Marum himfelf fet in leading his affiftants and direct- ing the operations of the pump, that he could prevail on them to face the danger, which they confidered as very dreadful.

Verbal procefs of Von Zach, on the method of extinguifhing fire.

The refult of the foregoing facts is, that in applying the method of extinguifhing fire, the circumftances to be obferved are thefe, that, to extinguifh the moft violent fire, it is only neceffary to wet the furface of the burning matter in the part in which the flames are feen, and that for this purpofe only a fmall quantity of water is needful, if the parts be wetted in the proper manner. In operations of this kind, therefore, particular attention muft be paid to throwing the water in fuch a way, that the entire furface of the burning part fhall be wetted and extinguifhed, and that in fuch a way that an extinguifhed part fhall never be left between two others which are in flames; for if attention be not paid to this, the heat of the flames burning here and there will quickly change the water with which the part has been wetted into fteam, and the whole will again take fire. In order, then, to extinguifh a fire in all cafes, no more water muft be thrown on the burning part than is needful to wet its furface; and this I conceive to be all that is requifite to extinguifh a fire whatever may be the circumftances of its origin.

VIII.

Report, prefented to the Clafs of Accurate Knowledge of the Academy of Turin, in the fitting of the 2d Nivofe, (Dec. 23, 1802) in the Year 11, on the Action of Galvanifm, and on the Application of this Fluid, and that of Electricity, in the Healing Art, by ANTOINE-MARIE VASSALI EANDI. *

Hiftorical introduction.

THE galvanic experiments, made the 22d and 26th of laft Thermidor, before a confiderable number of fpectators, by your affociates Giulio, Roffi, and myfelf, on the heads and trunks of three perfons who had been decapitated, the report of which you have caufed to be printed, excited many queftions refpecting this agent, and, by analogy, on electricity. Thefe two fluids, and their ufes, have furnifhed the fubject of the

* From the Journal de Phyfique, Germinal, An. XI.

the ordinary converfations of well-informed people; and, as always happens, when a difcovery is the topic, fome have exaggerated its advantages, and others have defpifed it. This diverfity of opinion led our affociate, citizen Charles Boffi, (qu. Roffi) to propofe two judicious queftions to me, which I have endeavoured to refolve in the following letter, written to him in the beginning of Vendemiaire. I could eafily have fwelled it to greater extent by additional reafons and numerous inftances of cures obtained by means of galvanifm and electricity, as well as with others of misfortunes, occafioned by thefe fluids, and with the conftant correfpondence which I have obferved between the moral powers, that is to fay, the ftrength of mind and courage of the victims of juftice, and the effects of the galvanic fluid upon their bodies; but thefe obfervations belong to the general report of the experiments we have made fince the 26th Thermidor; I fhall, therefore, confine myfelf to prefenting you the letter which I wrote at the above-mentioned time.

Queftions for difcuffion.

"You afked me, in one of our late academic fittings, whence it arofe that, after the number of experiments made by the moft able philofophers of the paft age, on the electric fluid, the difcovery of which is one of its moft brilliant conquefts, fo little is determined relative to the medical action of this fluid on the human body; and whether galvanifm appears really already to promife refults of more utility in the art of healing? I fhall inform you what my opinion is, or rather, I fhall fubmit to you the inferences, which the different experiments made by me alone, or in which I have affifted and witneffed, have enabled me to draw with more certainty than I ever dared to hope when I began ferioufly to engage in this purfuit.

Galvanifm confidered as a modification of electricity.

I confider galvanifm as a modification of electricity, which renders that fluid more active; as the fmall flame, feparated by the blow-pipe, is infinitely more powerful than the large flame from which it proceeds. I have read feveral experiments to the clafs, which fupport this parallel between electricity and galvanifm. Animals which had been only ftunned by the moft powerful explofions of the magic table, were killed in lefs than three minutes by galvanifm, which was far from being ftrong.

Proofs of its effects on animals,

The

The fluid of a pile of 25 pair of plates of filver and zinc, of the fize of a crown piece, feparated by difcs of pafteboard moiftened in water, faturated with muriate of ammonia, oxidates the metals by decompofing the water, while it is fcarcely perceptible to the fingers, and fhews but very fmall fparks. Brilliant electric fparks, which are fharply felt by our bodies, do not oxidate the metals, or decompofe the water, if they are not fo ftrong as to explode. On paffing the galvanic ftream through the body of a frog, it has happened that his humours have been decompofed, and I have feen him fwell fo prodigioufly, that he was unable to fink himfelf below the water, though perfectly lively; this I have never obferved in frogs tormented by exploding fparks. All thefe facts, to which I could add many others, prove the fuperior activity of galvanifm, when compared with electricity. Hence it refults, that the fluid from the electro-motor of Volta may be highly ufeful in cafes where the ordinary electricity is not fufficiently active. You are acquainted with fome of the experiments which I have made with my colleagues, Julio and Roffi; we have others ftill more interefting, which have determined phyfiological facts that have always been doubtful from a want of the means of verifying them. Laftly, we have tried the application of it in feveral diforders with the greateft fuccefs. Here are three inftances:

on metals and water,

on frogs.

Utility of Volta's electro-motor.

A lady, about 30 years of age, after a continuation of violent pain in the head, loft the fight of the right eye. Citizen Roffi was confulted on the complaint, and after a careful examination of this eye, which to appearance was as healthy and clear as the left, he concluded that it could be only a paralyfis of the optic nerve, or what is called a *gutta ferena*, which, allowing the patient to fee only a thick mift, increafed her misfortune, while it alfo contributed to derange the fight of the other eye, fo that fhe was in continual fear of falling, and was not even able to difcern with the right eye thofe objects which were within her reach. Roffi, being indifpofed, fent to me to apply galvanifm. I formed a pile of thirty pair of fuch plates as are mentioned above, and making ufe of gold cords for conductors, I caufed the galvanic ftream to enter clofe to the external angle of the eye, and to pafs out fometimes through the eye-brow; fometimes exactly at the opthalmic vein, which goes through the hole of the orbit; and fometimes clofe to the internal

Cure of a gutta ferena by galvanifm.

internal angle of the ſame eye. The operation was attended with ſome pain; it cauſed tears to flow abundantly; but after having received ſucceſſive galvanic ſhocks for half an hour, the ſight of the eye began to be reſtored. Being unwilling to fatigue the lady, and to allow time for nature to act, the operation was ſuſpended until the evening, when it was repeated for thirty minutes alſo. The following day the eye began to perceive the outlines of bodies. Having repeated the operation for three ſucceſſive days, ſhe not only was capable of ſeeing objects and the phyſiognomy of perſons, but even the balls of their eyes. Before this operation, in conſequence of a conſultation with doctor John Baptiſte Anſorni, firſt phyſician to the Hoſpital de la Charité, I had galvaniſed a girl of 27 years of age, of a melancholy habit, who, after ſome ſlight attacks, had a hemiparalyſis of the right ſide, which particularly affected the arm, the cheek, and the eye. The other ſymptoms had yielded to bleeding, and the uſe of ſuch other remedies as had been indicated, but the eye remained conſtantly fixed, with pains in its muſcles. Ten minutes of galvaniſation were ſufficient to excite an abundant flood of tears, and a diſcharge of water through the noſtril on that ſide which had been always cloſed after the above attack; the pains in the muſcles were alſo much alleviated; ſhe could even turn the eye to either ſide, but had great difficulty to raiſe or depreſs it, accompanied with a ſenſation of weight round the eye. The operation was repeated after a day's repoſe, which reſtored the complete freedom of motion to the eye, and freed it from every unpleaſant ſenſation.

Cure of a paralytic affection of the eye by galvaniſm.

Theſe two operations were performed in the preſence of ſeveral perſons, and executed almoſt wholly by citizen Heacinthe Carena, teacher of phyſic in the national college of Turin.

Cure of the hydrophobia by galvaniſm.

The advantages of galvaniſm will appear ſtill more deciſively to you by the cure of the hydrophobia, lately accompliſhed by citizen Roſſi, who will give a detailed and exact account of it in an intereſting memoir, on which he is employed.

A man who had been bitten on the middle finger by a mad dog came to conſult him for the pains he felt in his arm and back, and particularly in the finger, which had been bitten a month before. The actual cautery, applied to the finger, removed the pains, but in a few days they returned, accompanied by

4 ſymptoms

ſymptoms of hydrophobia. He could no longer bear the ſight of water without ſhuddering; an inflammation of the throat obſtructed his ſwallowing even chewed bread, and a violent deſire to bite appeared every inſtant.

In this ſtate he was brought to citizen Roſſi, who perceiving that he could not endure the ſight of water, or even of any ſhining body, prepared, in another room, a pile of 50 pairs of zinc and ſilver plates, ſeparated by 50 diſcs of paſteboard, moiſtened in a ſolution of muriate of ammonia; he then made uſe of ſmall ſtrips of moiſtened white brown paper for a conductor, on which he cauſed the naked feet of the patient to be placed, and at the moment he opened his mouth to bite, he thruſt in the end of the conducting-rod, which communicated with the other extremity of the pile. The patient ſuffered much during this operation, which, after ſeveral ſhocks, rendered him ſo weak, that he was unable to ſtand: being then extended on the floor, he was galvaniſed with facility. The operation produced a copious perſpiration. After ſome time, Roſſi cauſed the man to be carried home, with directions that he ſhould be brought again the following day, that the operation might be repeated. It was at two o'clock in the afternoon that the patient was galvaniſed, at ſix o'clock on the following morning he came alone to citizen Roſſi, to tell him he was perfectly cured, for he felt no pain, nor difficulty in ſwallowing, and had entirely loſt all averſion to water and other liquids: but no arguments could induce him to ſubmit to another operation.

In a few days after, ſome ſlight pains having cauſed him to dread a new attack of the hydrophobia, he returned to Roſſi, who, by galvaniſm, again removed all the ſymptoms. This cure was likewiſe performed in preſence of ſeveral people. The patient is poſſeſſed of ſuch ſenſibility, that more than a month afterwards he felt the ſenſation of galvanic ſhocks as far as his ſhoulders, which I could ſcarcely perceive beyond the third joint of the finger, and I am not among the leaſt ſuſceptible. From theſe eſſays you will perceive what benefits may be hoped from galvaniſm. I do not doubt that an agent of ſuch activity may be the means of preſerving many individuals from the grave, by applying it at the moment when an accidental cauſe has ſuſpended the functions of the vital organs.

Benefits to be expected from galvaniſm.

This will become more obvious from the explanation of the medical action of electricity on the human body.

Difference of opinion concerning electricity, arifing from its mifapplication.

Several celebrated writers have placed electricity among the moft certain and active remedies; others have demonftrated the inutility and even danger of this fluid as a remedy; each of them have fupported their opinion by well-afcertained facts.

Nothing is more eafy to be accounted for when we reflect on the very great number of thofe, who, in the application of electricity as a medicine, have only acted empirically without having confulted the nature of the diforder, or of the agent by which they intended to cure it. It is for this reafon that, in the memoir which will appear in the volume of the academy, I have recommended the greateft caution in the ufe of galvanifm, which, like electricity, may do a great deal of harm; and that I have afferted that this remedy, though very excellent in itfelf, has done more harm than good in confequence of improper application. I fhall not fpeak of the chimerical properties which, in the enthufiafm of novelty, have been attributed to electricity, fuch as that of conveying to the human body, by friction with them, the action of remedies fealed up in glafs tubes.

Electricity may do harm when injudicioufly applied.

It is notorious that it is the fate of every new difcovery to be fo over-rated, that its opponents are at no lofs for reafons to object to it, but after fome time things are brought to the proper level, eftablifhed by the more intimate knowledge of the agent. Thus thofe who are fufficiently acquainted with the properties of electricity, know how to diftinguifh the cafes in which it can be employed to advantage from thofe in which it would be injurious. Of ten patients affected with the fame complaint, who undergo a fimilar courfe of electricity, five may be entirely cured, and the other five fhall be very ill.

New remedies injudicioufly over-rated, &c.

Thofe who are cured extol electricity as the beft of remedies; thofe who fuffer fay that it only increafed their complaint; both fpeak from accurate experiments; both are right and wrong at the fame time from making the application too general, that is to fay, becaufe they do not diftinguifh the caufe of the diforder which requires to be oppofed by the ufe of electricity. I will explain myfelf: The fame complaint,

 for

for inftance *fciatica* *, may be occafioned either by obftructed humours, by their too great quantity, or by a defect in the reaction of the folids; it may alfo arife from organic defects, from a change in the humours, from a venomous, or as it is called an *acrid* principle, or from a peculiar virus which is found in the humours.

Thofe five who are affected from an obftruction in the humours receive the greateft benefit from electricity, which fets them in motion; the other five, who are tormented by a fciatica proceeding from vitiated humours will grow worfe under the electric treatment, which increafes the acridity of the humours, carrying off part of the water which kept the poifon diluted. This theory of the effects of electricity on the human body is founded on the nature of this fluid, and on the properties it manifefts in numerous experiments.

Conftant tendency of the electric fluid to an equilibrium, urged as the mode of its action.

The electric fluid tends conftantly to an equilibrium, and this tendency is fo ftrong, that it is feen to penetrate through the air to a certain diftance, and to fpread itfelf on idio-electric bodies. It is this tendency which caufes electrifed water to run in a continued ftream through capillary fyphons, whence it paffes flowly in drops, if the electrization is ftopped. It is by the fame tendency that electricity accelerates the circulation of the blood in animals †, and of the humours in vegetables. It is, laftly, by the fame tendency that when the electric fpark paffes from one conductor into another by connecting wires, if the cohefion of the body be not fufficiently great to prevent it, it takes with it, in its paffage, fome of the conducting particles, which ferve it as a vehicle. This property, which is manifefted by the ordinary effects of thunder, and of feveral experiments, fhews the caufe of the increafed evaporation of electrifed liquids, and the greater tranfpiration of animals and vegetables under fimilar circumftances. It is clear, therefore, that in every cafe of obftructed humours, if other fymptoms do not forbid, electricity will be a very good remedy; on the

* Doctor Balbis obferved to me, that all the fpecies of fciatica could be very well accounted for without having recourfe to the hypothefis of corrupted humours; I replied to my learned brother, that it was my defire to compare my theory with the chief theories of fciatic affections without attending to their probability.

† Van Marum feems to have afcertained that it does not produce this effect. See his account of the Haarlem machine.—N.

 contrary,

contrary, if the complaint proceeds from vitiated humours, or from a diluted virus being contained in them, then electricity, either by evaporating the diluting humour, or by caufing a greater change, will be hurtful. Hence it is evident, that electricity and galvanifm ought to be ufed with the greateft circumfpection, and that the nature of the diforder, as well as of thefe fluids, muft be confidered, before it can be decided, whether the application is proper or not. It muft alfo be obferved that this remedy, on account of its activity, like all other remedies, however good they may be, becomes dangerous when abufed.

An inftance of injury from an imprudent application of galvanifm.

I might bring many inftances of injury occafioned by the mifapplication of electricity, even in cafes where a fhort time before it had been indicated. But I fhall only produce one cafe relative to galvanifm. A young female had been cured of pains in the mufcles of her face by galvanifm; but after the cure, in confequence of continuing the application fhe did herfelf harm, which increafed by the galvanifation, and did not ceafe until fhe left herfelf intirely to the powers of nature, affifted by nourifhing food. The patient, who is not capable of forming an opinion on the ftate of his health, muft therefore confult an able phyfician, one who does not defpife nature and new difcoveries. By this caution he will never be hurt by the application of electricity or galvanifm, which as Boerhaave fays (*Elem. Chemiæ, pars.* 3. *proceffus* 198.) of another very active remedy: *mirè præftat in multis incurabilibus: at prudenter á prudenti medico, abftine fi methodum nefcis.*

IX.

Account of two Mufquets of peculiar Conftruction for the Purpofe of quick firing.—W. N.

Two mufquets belonging to the earl of Warwick.

THE two pieces are the property of the Rt. Hon. the Earl of Warwick, who has permitted me to examine and defcribe them: both have the name *Wm. Martindale, Londini,* in gold upon the barrel, and the fafhion of mounting them is little different from that at prefent ufed. This name might perhaps fix the time of their fabrication nearly, if the regifters of the Armourer's Company, or other fimilar books were to be confulted.

fulted. I fhould fuppofe them to have been made about a century ago. Neither of them is in good condition.

One refembles Lord Camelford's piftol.

One of thefe pieces is conftructed precifely on the principle, and with the fame parts, as the piftol belonging to Lord Camelford, which was defcribed in our laft Volume, page 250, but the workmanfhip is not fo good. The chamber piece is of brafs, and the barrel is 30½ inches long. I think the magazine would hold nine or ten balls.

Defcription of the other. It cocks and primes at once.

The fecond piece is on a different conftruction. It feems to have been invented before the other, probably by the fame perfon. The barrel, which is 39 inches long, has no breech pin, but is perforated clearly through. The ufual charge of powder and ball is lodged in a feparate iron cylinder, (Fig. 3, Plate VIII.) two inches and three quarters long, and of the fame bore as that of the piece itfelf, namely a little more than half an inch, or 0.54 inch. The metal of this cylinder is about one twelfth of an inch thick, and it is lodged for actual fervice in the pofterior part of the bore of the gun, which is enlarged for the purpofe of receiving it (fee Fig. 2, letter G.) The gun is provided with five of thefe cylinders, four of which are lodged in cells in the ftock ready for ufe, while the fifth is fuppofed to be placed in the gun itfelf. There is a touch hole in the cylinder which anfwers to another in the barrel that opens into the pan; and this laft is a cylinder fixed to the barrel itfelf, having an excavation on its upper furface for lodging the priming (B, Fig. 1 and 2.) Immediately behind the breech end of the barrel there is a receptacle in the ftock, or rather in the barrel itfelf (E), for lodging the charged cylinder previous to fliding it into the chamber of the barrel. It is to be obferved, that the metal of the barrel is continued back four inches farther than the termination of the bore, and that it is in this ftrong metallic part that the receptacle E is made. The lock itfelf is attached to the piece by having the part immediately beneath the fhutting face of the hammer, fitted upon the fixed cylindrical pan B, and fecured by an end fcrew fo as to move round upon that cylinder. So that it is to be underftood, that the lock plate can either remain in the ufual pofition, or may be brought up to a pofition in which its length fhall be rather more than at right angles with the length of the barrel, as in the line H. In this laft pofition the priming is given, and the piece cocked exactly by the fame means as in

Defcription of a mufquet for quick firing.

Lord

Defcription of a mufquet for quick firing.

Lord Camelford's piftol, excepting that here the lock itfelf performs the motion, inftead of the cylinder that carries the pan, which continues unmoved. In Fig. 1. the ftud D proceeding from the barrel reacts upon the hammer, and fhuts it at the fame time that the flat bar proceeding from the cock comes to reft on a notch in the barrel, and caufes it to acquire the pofition of full-cock. Fig. 2. is a fection of the barrel feen endwife, and of the lock raifed up. When the lock, and the piece to which it is fixed are returned to the ufual fituation, the receptacle E is filled by a folid piece of iron, which forms a portion of A C, and effectually refifts the recoil which the piece C would otherwife undergo. At A is a catch that ferves to fecure the lock and apparatus in the laft-mentioned pofition; but when the gun has been difcharged, and the fubfequent operations are to be refumed, that catch is drawn back, and the fpring C throws the appparatus of the lock a little way up, in order that it may be then raifed, and the empty charge-piece taken out and another put in, &c.

It may perhaps be confidered as loofe conjecture to fuppofe thefe guns were originally invented by the Marquis of Worcefter, whofe "Century of Inventions" was publifhed about 1655, and hath fince been reprinted by various publifhers, and in periodical works. Out of this work I copy the following, which will at leaft amufe the reader.

Accounts of inventions of quick firing by the Marquis of Worcefter.

"58. How to make a piftol difcharge a dozen times with once loading, and without fo much as once new priming requifite, or to change it out of one hand into the other, and ftop one's horfe."

This feems to have been the piftol of Lord Camelford before defcribed in our Journal, and the firft of the guns here mentioned.

"69. Another way as faft and effectual, but more proper for carabines.

"70. A way with a flafk appropriated unto it, which will furnifh either piftol or carabine with a dozen charges in three minutes time, to do the execution of a dozen fhots as foon as one pleafeth proportionably.

"71. A third way, and particular for mufquets, without taking them from their refts to charge or prime, to a like execution, and as faft as the flafk, the mufquet containing but one charge at a time."

I fup-

I ſuppoſe this laſt to have been the contrivance, of which the ſketch is given in Plate 8.

Accounts of inventions of quick firing by the Marquis of Worceſter.

" 72. A way for a harquebuſs, a crock, or ſhip muſquet, ſix upon a carriage, ſhooting with ſuch expedition, as without danger one may charge level, and diſcharge them ſixty times in a minute of an hour two or three together.

" 73. A ſixth way, moſt excellent for ſakers, differing from the other, yet as ſwift.

" 74. A ſeventh way, tried and approved before the late king (of ever bleſſed memory) and an hundred lords and commons, in a cannon of eight inches half quarter, to ſhoot bullets of 64 pounds weight, and 24 pounds of powder twenty times in ſix minutes; ſo clear from danger, that after all were diſcharged, a pound of butter did not melt being laid upon the cannon-breech, nor the green oil diſcoloured that was firſt anointed and uſed between the barrel thereof and the engine, having never in it, nor within ſix feet, but one charge at a time."

A Chineſe cannon with a number of ſeparate chambers.

The diameter of this piece is fully ſufficient to carry an iron ball of 64lb. I ſuppoſe this contrivance to have been the ſame as one which was uſed in China much later, *viz.* in the year 1725, in the third * year of the emperor *Yong-tcheng*, and made from a model preſented by the chief of one of the provinces. Its bore was about one inch, and conſequently it threw a ball of leſs than four ounces of iron, or ſix of lead. A receptacle was made in the hinder part of the barrel, ſimilar to the muſquet juſt deſcribed; but inſtead of the charge being lodged in the kind of iron cartridge, Fig. 3, Plate VIII. there was an actual breech piece for containing the charge, which piece was lodged in the receptacle when in ſervice, and could be placed or replaced by an handle like that of any other veſſel. The gun was provided with four of them. If the Marquis of Worceſter's great gun was of this deſcription, the heat would be chiefly produced in the ſhifting piece, and conſequently the gun would, as he ſays, continue cool. This ſhifting piece muſt have been very weighty, even without its charge, and little durable in its fitting.

Inventions of the Marquis of Worceſter for quick firing.

" 75. A way that one man in the cabin may govern the whole ſide of ſhip muſquets to the number, if need require, of 2 or 3000 ſhots.

* Memoirs concernant les Chinois, vol. 7. Paris, 1782.

" 76.

" 76. A way that againſt ſeveral advances to a fort or caſtle one man may charge fifty cannons, and ſtopping when he pleaſeth, though out of ſight of the cannon.

" 77. A rare way likewiſe for muſquetoons, faſtened to the pummel of the ſaddle, ſo that a common trooper cannot miſs to charge them with twenty or thirty bullets at a time, even in full career.

" When I firſt gave my thoughts to make guns ſhoot often, I thought there had been but one only exquiſite way inventable, yet by ſeveral trials and much charge I have perfectly tried all theſe."

X.

Of Rain. *By* Richard Kirwan, *LL. D. F. R. S. Preſident of the Royal Iriſh Academy, &c.* *.

Rain is not cauſed by refrigeration;

DIMINUTION of the temperature of air, replete with moiſture, below the degree at which its ſaturation takes place, whether this refrigeration were cauſed by rarefaction, or by the intermixture with colder air, has been generally ſuppoſed the cauſe of rain; but this hypotheſis in both its branches has been ſatisfactorily refuted: the firſt by Sauſſure † and the ſecond by De Luc ‡. And in fact refrigeration will indeed ſeparate moiſture from air cooled below the temperature neceſſary to hold it in ſolution. But this ſeparation will terminate, if gradual, in the production of *dew*, as already ſeen, or, if ſudden, in the production of a *cloud*, as in the experiment of Tornea mentioned by Maupertuis, but cannot nor has in any inſtance produced *rain*.

for this produces dew or clouds.

Rain is cauſed by the privation of electricity.

Rain is the immediate reſult of the union of the particles which form clouds; and this union is the conſequence of the ſubtraction of the electric atmoſpheres which keep them at a diſtance from each other; and this ſubſtraction is itſelf the conſequence either of the ſuperior attraction of better conductors or of the attraction or repulſion of other clouds through the cauſes mentioned in the firſt ſection of this chapter. The

* Iriſh Academy, 1802, p. 487.

† Hygrom. § 224.

‡ De Luc Idées de Meteorologie, p. 43, &c.

conſequence

confequence of the *attraction* of clouds is their *incorporation,* and the refult of their incorporation is the increafed volume of their conftituent particles, an increafe proportioned to the attraction that produced it; the increafed volumes, thus produced, form thofe drops whofe collection we call *rain.* The weight of thefe being fuperior to the refiftance of air, they neceffarily defcend, and the caufe of their different fize is thus clearly difcerned.

Developement of the effects.

The repulfion of clouds fimilarly electrified, and not greatly differing in magnitude, terminates in a bare increafe of diftance; but, if their magnitudes be much difproportioned, it may terminate in *attraction,* or at leaft in forcing the conftituent particles into clofer contact, and thus by increafing their magnitude effect the fame refult.

When the attraction takes place between clouds differently and highly electrified, and within what electricians call the *ftriking diftance,* the electric fluid is fet free, the coalefcence of the nubilous particles is more rapid and complete, and hence the large drops that follow flafhes of lightning, or even floods, where the quantities both of vapour and electron are confiderable, as between the tropics.

Upon thefe principles moft of the phenomena relative to rain appear to me eafily explicable; of thefe the moft remarkable are:

Phenomona of rain.

More copious but lefs frequent in low latitudes.

1. That rains are more copious but lefs frequent in the fouthern parts of our hemifphere not much elevated over the fea, than in the more northern latitudes. They are more copious when their productive caufes occur, evidently becaufe the quantity of fufpended vapour is much greater in the hotter than in the colder regions; but they are lefs frequent, becaufe the variations of wind in different directions which introduce and intermix clouds indifferently electrified are lefs frequent; this might be proved by inftancing the rainy feafons between the tropics, were it not that this illuftration would extend this paper to too great a length. Even in moderately elevated fituations between the tropics, if infulated and of fmall extent as the ifland of St. Helena, it feldom rains.

Moft copious though leaft frequent in fummer.

2. That, in the temperate latitudes, rains are alfo more copious, though commonly lefs frequent, in *fummer* than in *winter,* for the reafons already affigned. *Dry* fummers are then the confequence of uniform winds, from whatever quarter

ter they may blow, as *wet* fummers are of their variation, particularly if in oppofite directions, and if they reach heights fufficient to intermix the clouds that fubfifted during the reign of their antagonifts.

Southerly winds bring rain in Europe.

3. Southerly winds are moft frequently accompanied with rain, in moft parts of Europe at leaft, and probably in moft parts of our hemifphere; but *northerly* and *eafterly*, with clear, dry, and ferene weather. Becaufe foutherly winds are not only warmer, proceeding from warmer climates, but alfo more highly electrified than the foil of the colder countries into which they flow. Hence the copious vapours they contain are quickly deprived of part of their electron, and thus converted into clouds; but the fuperior ftrata of the atmofphere under which the fouthern air is introduced, not being fupported by air as denfe as that which fubfifted under them before their introduction, neceffarily defcend and mix with the inferior fouthern air; by this intermixture they are warmed, and deprive the clouds already formed and in its vicinity of part of their electron, or perhaps in conformity to the eleventh principle, they are themfelves deprived of part of their electron by thofe clouds, and the vapours they contain are thus converted into clouds; in either way clouds differently electrified muft be formed. Hence proceeds their gradual attraction to each other which terminates in thofe gentle fhowers that ufually accompany this wind. Northerly and eafterly winds on the contrary, proceeding from colder countries are lefs highly electrified than the foil of the countries they invade; and hence from the oppofite reafons to thofe juft mentioned they introduce ferene weather and a difpofition adverfe to nubification.

The reafons hitherto adduced to explain the different effects of thefe different winds, evidently arofe from an ignorance of the origin and progrefs of thefe winds. It was imagined that foutherly winds, flowing into colder countries were fuddenly cooled by an intermixture with the colder air of thofe countries, and that thus their vapours were condenfed into rain, yet, even fo, this intermixture could only produce clouds and not rain, but in fact this intermixture cannot take place, except with the fuperior and unmoved ftrata of the atmofphere, and thefe alone could not produce numerous clouds, much lefs copious rains; for the air of the countries into which thefe

foutherly

ſoutherly winds flow, muſt itſelf have flown northwards, before the more ſouthern air could enter upon them, as ſhewn p. 129.

Moreover, ſoutherly winds retain much warmth, and northerly winds are ſo much colder in the countries into which they are introduced, that their temperature cannot be ſuppoſed ſufficiently altered to depoſit much vapour in the one caſe, or diſſolve much of that already condenſed in the other; on the contrary, the warm ſoutherly wind ſhould diſſolve the clouds already formed, and the northerly, by their encreaſed cold, ſhould produce many more.

Hence electrical agency muſt of neceſſity be recurred to, though I do not doubt but it may be more correctly applied by perſons better verſed in electrical knowledge than I can pretend to be. Currents of air flowing in different directions at different heights in the atmoſphere, muſt undoubtedly be intimately connected with theſe effects, but with theſe we are at preſent too little acquainted.

4. That a diſpoſition to rain is generally connected with a diminution of the weight of the atmoſphere, as is a diſpoſition to ſerenity with the increaſe of its weight. Becauſe under the diminiſhed weight of the atmoſphere, the eruption of vapours both from land and water is much more copious, a diſpoſition highly favourable to nubification, and the clouds already formed deſcend lower, are more concentrated, and hence more diſpoſed to react upon and attract each other and thus produce rain. The increaſed weight of the atmoſphere muſt produce oppoſite effects and induce a diſpoſition adverſe to the production of rain. **The diminiſhed weight of the atmoſphere increaſes evaporation and cauſes rain.**

5. That more rain falls on the ſurface of the earth than on ſmall elevations above it, as from 30 to ſome 100 feet: ſee Phil. Tranſ. 1769, p. 361; and of 1771, 297; and of 1777, p. 256. This effect ſeems to me to proceed from the greater ſtillneſs and tranquillity of the air near the ſurface of the earth than at greater elevations. To prove this, it is only neceſſary to collect the rain that falls in moderate weather on both ſituations, with that which falls on both, in more ſtormy weather. If this explanation be juſt the difference between the quantities collected in both ſituations will be found greater in the latter than in the former caſe. This experiment I ſhall make, and communicate the reſult to the academy. **More rain falls on the ground than at ſmall elevations.**

6. That

Rain on mountains, and plains near them.

6. That the quantities of rain collected at the top of high mountains, and on plains about half a mile distant from those mountains are nearly equal, but in summer there falls somewhat more on the plains, and in winter somewhat less. Phil. Transf. 1771, p. 295. The greater quantity of rain collected in summer on plains appears to me to proceed from the cause just mentioned, the less disturbed state of the atmosphere; but the quantity of rain gained through the influence of this cause is often, in great measure, compensated by that arising from the condensation of fogs formed on the summits of mountains, particularly at night, when neither fog nor rain exist on the plians. But in winter, these mists being much more frequent and denser on the summits of mountains, the quantity of moisture which they deposit is far more considerable.

More rain on the westerly coasts of Europe.

7. That it rains much more on the western coasts of most parts of Europe, particularly if mountainous, than in the interior parts of those countries, or on the eastern coasts of the Britannic islands.

The cause of these phenomena is very obvious. Westerly winds are by far the most frequent in most parts of Europe; these flow from the Atlantic which bounds it, and generally convey marine clouds electrified differently from the soil or land over which they flow, as also from that of the higher clouds under which they reign. Hence proceeds their mutual attraction, and thence rain. This effect must take place principally on the western coasts; when they proceed further, this different electrical state must either cease or be diminished. When the coasts are mountainous, these mountains quickly absord the electric matter contained in the western blasts, and by collision, condense the vapours they contain, first into clouds, and finally into rain. Hence it often happens that westerly winds, particularly in summer, produce no rain, either because they introduce no clouds, or meet with none differently electrified.

Countries where it never rains.

Peru.

8. That in some countries it scarce ever rains.—This arises from local circumstances, as is apparent in the following instances: 1. It never rains on the plains of Peru from the gulf of Guyaquil, nearly under the equator, up to latitude 23° south, nor is thunder ever heard there, though these plains border on the Pacific Ocean, but they receive a slight dew every night. Bouguer, fig. de la Terre XXIII. 2 Ulloa's Mem.

Mem. p. 157. 2 Phil. Tranf. Abr. 132. Plainly from the following reafons: Thefe plains are entirely fandy, and, confequently, emit very little vapour, being foon parched by the heat that there prevails; confequently the intermixture of marine vapours can produce no effect. Again, the clouds in thefe tracts are elevated to a great height, and are attracted by the electrical agency of the Cordellierres that border on thefe plains, to their lofty fummits, and there produce copious rains; hence alfo the fandy and extenfive defarts of Arabia and Africa are feldom refrefhed by rain. 2. It fcarce ever rains in Egypt, particularly in Upper Egypt. Now it is to be obferved, that Egypt is fo fituated betwixt lofty mountains that no wind can enter it without paffing over them, but the northerly winds, which iffue from the Mediterranean; for a foutherly wind muft pafs over the mountains of Abyffinia; an eafterly, over thofe that intercede between the Red Sea and the Nile, and proceeding from the Defarts of Arabia, can convey little or no vapour; and wefterly winds muft pafs over the Defarts of Africa and Mount Atlas. Now the northerly wind does not begin to blow until the month of June, when Egypt is fo fcorched as to emit fcarce any vapour, and the few clouds it may convey are attracted by the mountains of Abyffinia. Towards the middle of June the inundation of the Nile, it is true, commences, and then, as the northerly wind ftill continues, perhaps rain might be expected, but little attention being paid to it then, we are not informed whether any falls or not, perhaps the clouds then alfo pafs to the mountains of Abyffinia, whither this wind conveys them, and which they deluge with rain; all other winds depofit their moifture on the reverfe of the mountains they pafs over.

Arabia. Africa. Egypt.

Prognoftics of Rain.

When the barometer falls, and the hygrometer rifes, *rain* is announced.

Prognoftics of rain.

When the barometer rifes, and the hygrometer falls, we may expect fair weather, if farther changes do not appear in thefe inftruments, as fometimes there fuddenly do.

If the barometer falls and the hygrometer alfo, *windy weather* will probably follow—particularly if the barometer falls much below its natural height, which in Dublin is from 29,9 to 29,98.

Again,

Again, in the morning the hygrometer is generally higher than at noon, by reafon of the difference of temperature; but if it ftands *lower at noon* in a greater proportion than the difference of temperature demands, it prognofticates *fair weather.*—On the contrary, if at noon it be higher than it ftood in the morning, *rain* may be expected. Sauff. Hygr. p. 356.

To forefee the Rife or Fall of the Barometer in Day-time.

Prognoftics of change in the barometer.

Obferve it at feven o'clock in the morning, and afterwards at nine and at ten. If it remains fteady, its next motion will probably be *downwards.* So alfo if it falls within that interval of time, the probability is, that it will *fink* ftill lower. But if it rifes within that interval, the chances of a greater rife or of a greater fall are equal.

Again, obferve the barometer at one in the afternoon, and again at three; if it remains unmoved, it is probable that it will *rife,* but if it has fallen, the chances of a farther rife or fall are equal.

XI.

Obfervations and Experiments undertaken with a View to determine the Quantity of Sulphur contained in Sulphuric Acid; and of this latter contained in Sulphates in general. By Richard Chenevix, *F. R. S. and M. R. I. A.* (Irifh Acad. 1802.)

Uncertainty in the analyfis of pyrites from the combuftion of the fulphur.

IN a paper which I had the honour to prefent to the Royal Society of London, and the fubject of which was the analyfis of fome arfeniates of copper, and of iron, I had occafion, in examining many pyrites, matrices of thofe ores, to remark the very great inequality which prevailed in the refults of repeated experiments, made with a view to determine the proportion of fulphur. But I foon perceived, that the inaccuracy was caufed by a partial combuftion and acidification of the radical, through the means of the nitric acid, employed to diffolve the ore.

The burned fulphur, or fulph. acid, precip. by barytes.

Having therefore, in the ufual manner, afcertained what quantity of that ingredient remained untouched, I was forced to feek the reft of it in the liquor, which had wafhed the various precipitates. To obtain it, I poured a folution of nitrate of

of barytes into thofe wafhings, when all the other fubftances had been carefully feparated, and was thereby enabled to precipitate, in a ftate of purity, the fulphate of barytes, formed by that earth, and by the portion of fulphur originally acidified, in the firft treatment of the ore by nitric acid. To come at the knowledge of the proportion of fulphur, contained in a given quantity of fulphate of barytes, I had recourfe, in the firft inftance, to the quantity of fulphur, faid by Lavoifier to be contained in fulphuric acid, and, in the next, to the proportions of the latter, announced in the fynoptic tables of Fourcroy, as entering into the compofition of fulphate of barytes. According to the former of thofe chymifts, 100 parts of fulphuric acid contain 71 of fulphur and 29 of oxygen; and again, in adopting the proportions of the latter, we have 33 per cent. of acid, in fulphate of barytes. But if 100 contain 71 of fulphur, 33 muft contain 23.43. Confequently, for every 100 parts of fulphate of barytes, I was to allow 23.43 of fulphur. But, by the refults of my analyfes, I had fuch quantities of fulphate of barytes, as induced me to doubt the accuracy of one or other of the ftatements, by which I eftimated the quantity of real fulphur contained in the ore.

Deduction of the fulphur from the acid, by the received proportions did not agree with the experiments.

No perfon is better acquainted than our celebrated prefident, with the many difficulties that occur in the analyfis of falts in general; particularly with regard to the quantity of real acid they may contain. It has been a work of trouble to the ableft chymifts, and they have not always agreed in their refults. The proportions, announced by Fourcroy, may therefore be doubted, in common with thofe of the other learned operators, to whom I have alluded.

The real quantity of acid, produced by the combuftion of any acidifiable bafis, can be determined by one or other of the following methods only: by direct combination, in fome falt, the proportions of which are already known; or by obtaining, in a ftate perfectly free from water, the acid refulting from fuch combuftion. To the former method, the general objections againft all analyfis of falts muft apply. The latter is ftill more defective. It is by no means certain, that we have ever yet obtained any acid, in a ftate of perfect ficcity, unlefs we except the phofphoric and the arfenic; for even the cryftallized vegetable acids retain a portion of water in their cryftallization. It is not that I abfolutely deny our having obtained them

The real quantity of acid formed by burning its radical, determinable by combining it in fome known falt, or elfe by obtaining the acid dry. The firft is uncertain from complexity; the latter in moft cafes impracticable.

them ſo; but I ſay merely, that we have no proof. It would indeed be ſetting narrow bounds to the perfection of nature, to aſſert, that no combuſtible body could, when ſaturated with oxygen, aſſume, of itſelf, the ſtate of liquidity; or that the oxide of the particular ſubſtance, called hydrogen, muſt be preſent to confer that property. Doubtleſs, ſulphuric acid may, as well as water, contain, in itſelf, ſo juſt a proportion of ſpecific heat, as to remain liquid at the temperature of our globe, and under the preſſure of our atmoſphere. But, both water and ſulphuric acid being eaſily volatilized, and having a powerful affinity for each other, it is not eaſy, if even poſſible, by diſtillation, to ſeparate them with ſufficient accuracy, in experiments of delicate inquiry. A ſecond ſource of error, therefore, remained open in this branch of the calculation, which gave the proportion of 23.43 of ſulphur, as that contained in 100 parts of ſulphate of barytes.

Exp. 100 ſulphur was acidified by nitric acid;

However, before I would allow myſelf to call in queſtion ſuch authorities as thoſe I have quoted, I inſtituted the following experiments: In a tubulated glaſs retort, I put 100 parts of purified ſulphur, and poured upon them ſtrong nitric acid. A quilled receiver, plunging into a Woulfe's apparatus, was adapted to the retort; and, all being well luted, I proceeded to diſtil. The liquor, which came over, was poured back ſeveral times upon the ſulphur, until the whole was diſſolved. The water, which had come over, and that, through which the nitrous gas, produced during the operation, had paſſed, were eſſayed for ſulphureous acid, and no traces of it could be found. No ſulphur had been volatilized; therefore no ſuſpicion could remain, that all was not converted into ſulphuric acid. The liquors, which were in the various parts of the apparatus, were united; and to them was added a ſufficient quantity of nitrate of barytes. The whole was evaporated gently; becauſe, though I am well acquainted with the very little ſolubility of ſulphate of barytes, I well know that nitric acid will retain a ſmall portion of it, particularly when formed in a liquor where that acid abounds. In a firſt experiment, I obtained 694 from 100 of ſulphur, in a ſecond, 348 from 50, and in a third, 347 from the ſame quantity. But the ſimple rule of three reduced theſe quantities to 14.6, or 14.4 per cent. of ſulphur, contained in ſulphate of barytes; a difference wholly to be neglected. If, therefore we take

and the acid thrown down with nitrate of barytes.

Reſults, that 100 parts ſulphate of barytes contain 14.5 ſulphur. and alſo the oxygen.

14.5 as the average, for the quantity of ſulphur contained in 100 parts of ſulphate of barytes, we ſhall not be far from the truth. From the accordance of theſe experiments, repeated and varied, I have now no doubt, but concerning the ſource where I was to ſeek the error, which gave 23.43 as the juſt proportion.

To aſcertain this point, I operated in the following manner, I prepared ſome lime, as pure, I believe, as chymical means can procure it. I digeſted white marble in muriatic acid; and, by leaving an exceſs of the earth, was certain, that by the ſuperior affinity of lime for that acid, nothing elſe had been taken up. Upon trying the ſolution with ammonia, no precipitate took place. By means of carbonate of potaſh, I ſeparated the lime in the ſtate of carbonate; and, after well waſhing the precipitate, expoſed it in a platina-crucible to a violent heat, till the weight no longer diminiſhed. I am acquainted with no more efficacious method to prepare lime, fit for the delicate purpoſes of ſcientific chymiſtry.

I. Inveſtigation. *Pure lime was had by ſolution of marble in muriatic acid, then precip. by carb. of potaſh, and waſhed and calcined.*

One hundred parts of this lime were diſſolved in dilute muriatic acid, in the ſame platina-crucible, previouſly weighed; and then ſulphuric acid was added in ſufficient quantity. Sulphate of lime was precipitated; and the veſſel was expoſed to a heat, at firſt gentle, to evaporate the liquor; and then, by degrees, raiſed to a temperature, which could expel every thing but the combined ſulphuric acid, and leave the ſulphate of lime completely calcined. The crucible with the ſalt was then weighed and the augmentation was 76.—It appears to me, that, if we admit (and I ſee no reaſon that we ſhould not admit it) that calcined lime and calcined ſulphate of lime are wholly exempt from water, it muſt be clear, that the 76 additional weight were ſulphuric acid; and, that the ſulphuric acid muſt in this ſtate, more than in any other, approach nearer to what may be termed, abſolutely real acid. One hundred parts of calcined ſulphate of lime contain therefore

II. 100 p. lime diſſ. in mur. acid, were precip. by ſulph. acid, and the mur. acid and water driven off by ignition.

The dry ſulphate was 76 heavier, which were dry acid.

Lime	- - - - - -	57
Sulphuric acid	- - - - -	43
		100

Component parts of ſulphate of lime.

By the former experiments (thoſe made upon ſulphur converted into acid, and then united to barytes) we had the quantity of ſulphur, contained in ſulphate of barytes. By the latter

III. 100 parts ſulph. of lime were diſſolved in water

by the addition of oxalic and a little mur. acid.

latter (thofe made by directly combining lime with fulphuric acid) we had the proportion of real acid, contained in calcined fulphate of lime. Confequently, by knowing the ratio, that fulphate of barytes bears to fulphate of lime, with regard to the acid in each, we fhall arrive at the knowledge of the quantity of fulphur, contained in real fulphuric acid. For this purpofe, I attempted to diffolve, in water, 100 parts of fulphate of lime. But finding, in this method of proceeding, a confiderable inconvenience arifing from the great quantity of liquor, neceffary to effect the folution of that falt, I had recourfe to the following expedient. Upon 100 grains of calcined fulphate of lime, I poured fome oxalic acid, which attracts the bafis with an affinity fuperior to that exercifed by fulphuric acid. Oxalate of lime was here formed; but oxalate of lime is foluble in a very fmall excefs of any acid. A little muriatic acid operated a complete folution; and thus a great quantity of fulphate of lime required but little water to diffolve it. Into this liquor, muriate of barytes was poured, and fuffered to remain fome time, gently heated. By thefe means, any oxalate of barytes, that might have been formed, was retained in folution, by the original excefs of acid; and the entire quantity of fulphate of barytes was depofited. Of the exactnefs of all thefe methods, which I ufed, as the inftruments by which I afcertained thefe refults, I convinced myfelf by various preliminary experiments. After the ufual filtration, wafhing and drying at the gentle heat of a fand-bath, I obtained in one experiment 185, in another 183, and, laftly, in another 180. This difference does not exceed the limits of what all perfons, converfant in analytic chymiftry, will allow to experiments of this nature. We may therefore take 183 as the mean proportion; confequently, we fhall fay, that 183 of fulphate of Barytes contain the fame quantity of fulphuric acid, as 100 of fulphate of lime; and 183 : 43 : : 100 : 23.5. Therefore 23.5 is the proportion of acid in 100 of fulphate of barytes. But we have before feen, that 14.5 of fulphur, acidified by nitric acid, form that portion of fulphuric acid, contained in 100 of fulphate of barytes: viz. 23.5. We muft now fay, that 23.5 : 14.5 : : 100 : 61.5, and the fourth term will be the proportion of fulphur = 61.5, which combined with 38.5 of oxygen will form 100 of real fulphuric acid.

muriate of barytes being added, threw down the acid in fulph. of barytes weighing 183. This acid was known to be 43 parts.

The proportion in 100 parts barytes is therefore 23.5 acid, and the fulphur in this, by the firft exp. was 14.5. Real fulph. acid contains 14.5 fulph. and, the reft = 9.0 oxigen: or $61\frac{1}{2}$ to $38\frac{1}{2}$ = 100.

In

In neither of the proportions, whether it be of the acid contained in the ſalt, or of the combuſtible baſis contained in the acid, do I agree with the two chymiſts whom I have quoted. This juſtly excited ſome doubts in my mind, and led me to repeat my experiments. Nor ſhould I yet be thoroughly ſatisfied, If I could not, upon other grounds, than by ſuppoſing inaccuracy in them, account for the apparent differences. We muſt ever expect to ſee the errors of our predeceſſors corrected by men, much inferior in abilities; but who, by poſſeſſing more certain means, ſupply the want of genius and invention. At the time in which the experiments were made, that determined the proportion of 33 per cent. of ſulphuric acid in ſulphate of barytes, it was not known that we had never obtained any barytes pure; and that a conſiderable portion of carbonic acid reſiſted the action of every degree of heat that had been applied to carbonate of barytes. The fact was, I believe, firſt obſerved by Pelletier; but the method of avoiding the inconvenience was pointed out by Vauquelin. He decompoſes nitrate of barytes by lime, and a moderate degree of heat is ſufficient to expel all the acid and the water. The chymiſts, I have mentioned, performed ſynthetic experiments, by combining, directly or indirectly, ſulphuric acid, and ſuch barytes as they imagined to be pure. The conſtant ſimilarity of their reſults is ſufficient to prove the accuracy of their operations; but working upon an impure ſubſtance, they muſt have been contented with a ſimilarity of error.

Former experiments of others were inaccurate, becauſe barytes had not then been obtained pure,

Three cauſes may exiſt which are capable of accounting for any variation, whether in *plus* or in *minus*, that might have appeared in the experiments, by which Lavoiſier determined the quantity of ſulphuric acid, obtained by the combuſtion of ſulphur in oxigen gas.

and becauſe Lavoiſier, in burning ſulphur in oxigen,

1ſt. A part of the ſulphur may be volatilized during combuſtion.

might have volatized ſome,

2d. All the ſulphur may not be converted into ſulphuric acid; but part may remain in the ſtate of ſulphureous acid.

and imperfectly acidified ſome,

3d. In rectifying, ſome acid may come over along with the water; or, *vice verſa*, ſome water remain with the acid. Theſe conſiderations will excuſe me for having propoſed a doubt where the authority of ſo great a man exiſts againſt the experiments which I have related.

and been embarraſſed by water in rectifying.

Expts. of Taſſaert, Thenard, and Guyton.

The method which I had uſed to aſcertain the quantity of ſulphur in an ore, had been practiſed by Taſſaert, (*Annales de Chimie*, No. 82: Analyſis of Cobelt from Tunaberg) but he calculated according to the proportions of Lavoiſier and of Fourcroy. In another paper by Thenard, (*Annales de Chimie*, No. 96.) he ſtates the proportions of ſulphuric acid, obtained by treating ſulphur with nitric acid, to be

Sulphur - - - - -	55.56
Oxygen - - - -	44.44
	100.00

but in the extract given by Guyton in that No. of the *Annales de Chimie*, the mode of operation is not deſcribed. Calcined ſulphate of barytes is eſtimated in the ſame paper to contain

Barytes - - - - -	74.82
Sulphuric acid - - - - -	25.18
	100.00

which proportions are as near to what I had found as can be expected; for ſulphate of barytes does not contain more than 3 *per cent.* of water of cryſtallization, and they muſt be deducted from the quintal.

From the determination of the acid in an inſoluble ſulphate, it becomes eaſy to extend it to others.

Having determined with accuracy the proportion of acid in any inſoluble ſulphate, it is eaſy to proceed to the determination of that contained in any other ſulphate. The docimaſtic art or analytic chymiſtry in general, cannot however expect to derive ſuch advantages from the knowledge of ſoluble ſalts, as of thoſe, which, from their inſolubility, may be uſed with accuracy in delicate experiments to determine the proportions of the conſtituent parts of bodies. But if, with this mode of operating, we compare the quantities of real acid, ſaid (in thoſe excellent tables with which Mr. Kirwan has enriched the ſcience) to be contained in ſulphuric acid of different ſpecific gravities, they will ſerve as proofs of their mutual validity; and perhaps demonſtrate, that ſulphuric acid, as well as water, and ſome other bodies, may of itſelf enjoy liquidity, at the temperature and preſſure which act upon our globe.

SCIENTIFIC

SCIENTIFIC NEWS.

Improvement on Lamps.

A VERY great improvement in the conſtruction of lamps and reflectors, has been lately made by Mr. Nicholas Paul of Geneva, who in conjunction with Mr. Smethurſt, an eminent lamp contractor has lately made a public experiment, by illuminating the upper part of New Bond Street. New lamp of Mr. N. Paul.

Fifteen only of the new lamps with reflectors, were ſubſtituted in place of more than double that number of common ones; the effect of which was, that the ſtreet was enlightened with at leaſt twice the uſual quantity of light.

This effect is produced, not by the combuſtion of an extra quantity of oil, but by the ſcientific conſtruction of the apparatus; the lamp being formed upon the principles of the beſt air furnace; ſo that the whole of the combuſtible material employed, is converted into light and heat, without ſmoke; and that portion of light which would have aſcended and been loſt, is diſtributed to thoſe ſituations where it is required, by means of reflectors ſo formed as to diſtribute the ſame uniformly over the illuminated ſpace, and to obliterate the ſhadow that would be formed by the body of the lamp.

Theſe lamps and reflectors are not merely adapted to lighting the public ſtreets, but by various modifications, are accommodated to every circumſtance where lamps or candles can be made uſe of *.

Improvement on the Mode of Watching Cities.

Samuel Day, Eſq. of Charter-houſe, Hinton, in Somerſetſhire, in the commiſſion of the peace for the county, has directed his attention to the application of a mechanical check upon the diligence and preciſion of watchmen, labourers, and all other claſſes of men, whoſe duty requires that they ſhould attend at certain places at appointed times. His own account of this intereſting arrangement is as follows †. Mr. Day's watchman's regulator.

* I have received the above from Mr. Paul, but expect to give a fuller account from my own examination, in our next Number. W. N.

† From a printed paper with which he has favoured me. W. N.

From

Mr. Day's watchman's regulator.

From the concurrent teſtimony of many Individuals, the preſent ſyſtem of watching cities is languid and inadequate: no houſe is ſecure, when depredation is determined on, or if there be any ſecurity, it is more from the means taken within, than from the watching without. Magiſtrates have ſeen and pointed out the defects in the ſyſtem, and have at times applied what means they could to counteract the evils ariſing from thoſe defects, but their means have been ineffectual: and houſeholders have been obliged to ſubmit to their riſques with no other conſolation but that of thinking, that though the plan of watching was bad, it was better than none. In attentively conſidering the plan, the defects ſeem to reduce themſelves to the following heads; *1ſt*, The too long intervals which watchmen take, between their going their rounds—by which it appears that conſidering any individual houſe, that houſe has not the benefit of actual watching more than ten minutes through the whole night.

2ndly. The Watchman's call of the hours,—from which no ſervice ariſes to any, but to the depredators of the night, as is obvious to any one who reflects, that of the many hundred houſebreakings and ſtreet-robberies committed in London in the year, how few of the depredators are detected or taken by the watchmen themſelves, from no fault perhaps of theſe laſt, but becauſe the thieves have taken advantage of the watchman's repoſe in his box, and what is more, of the notice which he gives, by vociferation, of his diſtance or approach, by which they haſten, or delay their attack, or carry off their plunder accordingly.

3dly. The uncertainty of the watchman's doing his duty— who either from intoxication, drowſineſs, or indolence, or induced by the badneſs of the night, may miſs his rounds, without detection.

4th. The uſe of the lanthorn, which anſwers no purpoſe but that of adding to the ſignal of the watchman's approach. And laſtly the uſe of the watch-box, which anſwers no end but that of promoting drowſineſs, and perhaps diſeaſe, from the chills which are increaſed by inaction in a cold damp houſe.

For theſe defects it will be aſked what effectual remedy can be propoſed? To double the number of Watchmen would be the moſt obvious, but is out of the queſtion when we conſider the

Mr. Day's watchman's regularor.

the enormous expence which would attend it, and is unneceſſary when we reflect that the uſual number of men employed will be ſufficient, if any mode can be adopted of fixing and aſcertaining their vigilance. The inſtrument called the *watchman's noctuary*, or *labourer's regulator*, offers a mode for this purpoſe. By one of ſuch being placed at each end of a watchman's round, it will be aſcertained how the man continued his movements through the night, to a nicety of ten minutes at any period of the watch—and the ſlighteſt irregularity or omiſſion will be viſible the next morning to the inſpector or conſtable, whoſe office it ſhall be to open the machine. The teſt of regular and well-ſuſtained vigilance is given by the Watchman's dropping a token as he paſſes, every half hour, quarter, or half quarter, into a receiver or cell; each half hour or quarter preſenting it's own cell to receive the ſame, and each cell, like time itſelf, irrecoverable when paſſed. No trick or fraud on the watchman's part can counteract the movement of the horizontal wheel formed of theſe cells, and completing a revolution once in twelve hours. He has no command over it, and each cell (as it moves under the receiver) will be a kind of ſpeaking witneſs of his diligence and fidelity in going his rounds, anſwering the next morning to the exact periods he either was or ought to have been there.

By this means the calls of the Watchmen, which were only inſtituted for the purpoſe of his giving notice of being on his duty, will be ſuperſeded; and a conſiderable expence of animal exertion will be ſaved to the individual, which might better be converted into that of going his rounds twice, where he now only goes once. Warnings to the nightly thief of timely attack or retreat will likewiſe be taken away, and if inſtead of an open the watchman was to carry a dark lanthorn, the robber would have no ſecurity whatever in calculating the moment of his depredation, and might be detected in the very outſet of his attack, as the ſlighteſt ſound would alarm the watchman walking in ſilence, and not drowning diſtant noiſe by that of his own voice.

Of the objections to this new mode of ameliorating the watching of cities, the only one ſeems to the expence of the time pieces,—and conſidering the number which the larger pariſhes will have occaſion for, this expence will be important;

Mr. Day's watchman's regulator.

portant *; but let it be confidered that it will not amount to more than three pence in the pound of a rate on houfes, and that the firft will be the fole expence—probably to be faved by diminifhing the number of patroles to one half (or lefs) of what they now are. But trifling indeed will be the expence when compared with the loffes fuftained by the public in depredations, which, according to a late work on the police of the metropolis, amounts to two millions and upwards.

The beft fituation for thefe machines will be at each end of a watchman's round, perhaps certain rounds will require three. They ought to ftand in a convenient recefs in the ftreet, fecured by rivetings of iron, or let into a wall, or placed on a ftrong bracket within the iron railing of an area; and, if the dial plates were fuffered to appear, would be ufeful in the day as well as in the night: as an eight-day clock it would require no attention to it's movement but once a week, and the morning infpector might attend to the flight duty of winding it up.

The annual expence of keeping it in repair is too trifling to be taken notice of.

I have to remark that the fame machine will anfwer in cuftomhoufes, warehoufes, banking-houfes, docks, and every place where vigilance, to be ufeful, muft be exact.

Some Account of a pretended new Metal offered for Sale, and examined by RICHARD CHENEVIX, ESQ.

Interefting paper on the fo called palladium.

AN extremely curious and interefting paper by R. Chenevix, Efq, was read on Thurfday the 12th, and the following week, at the Royal Society. It purported to be an inquiry into the properties of a fuppofed new metal called *palladium*, or *new filver*.

The manner in which it was offered to the public.

This fubftance announced to the public in a fmall printed paper, partially circulated, was fold at an extravagantly high price. The notice is as follows; it is given *verbatim*, as the language in which it is written is not unworthy of attention †.

* It is prefumed that each time piece will amount to not lefs than twelve guineas, and each round will require two. D——. I apprehend they may be made for much lefs than half the money. W. N.

† I received a fmall piece by the poft. The piece of the fpecimen was at the rate of about one fhilling per grain.

PALLADIUM

PALLADIUM, OR NEW SILVER,

Has theſe Properties among others, that ſhew it to be a new noble Metal.

1. It diſſolves in pure ſpirit of nitre, and makes a dark red ſolution.

2. Green vitriol throws it down in the ſtate of a regulus from this ſolution, as it always does gold from *aqua regia.*

3. If you evaporate the ſolution you get a red calx that diſſolves in ſpirit of ſalt or other acids.

4. It is thrown down by quick-ſilver and by all the metals but gold, platina, and ſilver.

5. Its ſpecific gravity by hammering was only 11.3, but by flatting as much as 11.8.

6. In a common fire it tarniſhes a little and turns blue, but comes bright again, like other noble metals, when ſtronger heated.

7. The greateſt heat of a blackſmith's fire will hardly melt it.

8. But if you touch it while hot with a ſmall bit of Sulphur it runs as eaſily as Zinc.

It is ſold by Mrs. *Foſter,* at No. 26, *Gerrard Street, Soho, London.* In ſamples of five ſhillings, half a guinea, and one guinea each.

Immediately on the receipt of this paper, Mr. Chenevix went to the place where it was advertiſed to be ſold, and bought a piece which he ſubmitted to the trials propoſed by the author of the impoſition. Mrs. Foſter, it appears, is only the vender, and totally unacquainted with the perſon who brought the metallic ſubſtance and the printed paper to her houſe **Examined by Mr. Chenevix;**

In every reſpect but one, the aſſertions in the paper Mr. Chenevix found to correſpond with his own experiments. The ſpecific gravity he found to vary in different pieces from one to two per. cent.

The reſemblance which, in many of its properties, this ſingular ſubſtance bore to Platina, induced a ſuſpicion in the mind of Mr. Chenevix that the laſt mentioned metal formed a part of the compound, if indeed it was a compound body; but the eſſential characters of difference were ſo ſtriking and **Suſpicion that it might contain platina.**

 numerous

numerous as almoſt entirely to remove this doubt, or to convince him that palladium was really a ſimple ſubſtance; as its concealed author pretended, and not an impoſition upon the public, as Mr. Chenevix has now proved it to be.

It is platina and mercury.

It appears then, from Mr. Chenevix's experiments, that this pretended ſimple metal is a combination of platina and mercury; and it muſt excite not only aſtoniſhment, but humility in the cultivation of natural knowledge, to find that a combination of two metals, each upon ſo high a ſpecific gravity as thoſe which are combined in palladium, ſhould produce a compound, the ſpecific gravity of which is leſs than that of the leaſt weight of the component ſubſtances; and, moreover, that mercury whoſe affinity for caloric had hitherto been found to baffle all attempts to deſtroy its volatility; could, when combined with platina, become as fixed as any metallic ſubſtance, with which we are acquainted. But nature laughs at our theories, and forces us from time to time, by extraordinary diſcoveries of this ſort, to acknowledge that we are her vaſſals, and that we ſtrive in vain to bring her within our laws.

Proceſs for compoſing it.

Without entering into any detail of the contents of this paper, which is intereſting in many other reſpects, as well as in the diſcoveries which it announces, we ſhall ſhortly ſtate one of the means, ſeveral being given, by which Mr. Chenevix accompliſhes the production of palladium; or, at leaſt, of a ſubſtance having every chemical or phyſical property of that ſuppoſed metal. He diſſolves platina, procured from the ammoniacal ſalt, in nitro muriatic acid. To this he adds twice its weight, at leaſt, of red oxide of mercury, but if the acid be not ſaturated he adds more till the whole liquor is perfectly neutralized. This combination of muriate of platina and mercury, is then to be poured into a ſolution of green ſulphate of iron, and the black metallic precipitate which is thereby formed is, when collected, and the exceſs of mercury driven off, at a red heat, fuſed in a Black's furnace. It melts into a metallic button at a white heat, and becomes palladium; a compound as it ſeems, of two parts of platina to one of mercury.

Variations in the compound.

Mr. Chenevix finds it difficult always to combine with platina the ſame quantity of mercury; and the ſpecific gravity of his palladium varies like that which is ſold by Mrs. Foſter;

4 but

but he has found, when the experiment completely ſucceeds, that palladium, containing one third of mercury, is of the ſpecific gravity of 11.5,—when it contains one fourth of that metal, the ſpecific gravity is one part higher; and the higher the ſpecific gravity, the greater is the proportion of platina, and the leſs the ductility of the compound. Above 12.5, it ſo far ceaſes to have the properties of palladium, that it is no longer ſoluble in nitric acid.

The remaining particulars of this highly intereſting paper we muſt defer till after its publication by the Royal Society.

A new Method of Painting, propoſed by M. CARBONELL, *a Spaniſh Phyſician, capable of being advantageouſly ſubſtituted for Painting in Diſtemper* *.

THE diſagreeable ſmell perceived on entering rooms newly painted in diſtemper is univerſally known. It is only after having left theſe rooms expoſed to the action of the air, that they are habitable. A proceſs which remedies theſe two inconveniences, deſerves therefore to be adopted, and it is with that view that we inſert that lately publiſhed by *M. Carbonell*, a Spaniſh phyſician.

Bad ſmell of painting in diſtemper.

The mode of operation, deſcribed by the author, is very ſimple, it conſiſts in ſubſtituting the ſerum of the blood of oxen, inſtead of the ſolution of glue commonly uſed to mix up the colouring matter.

Simple proceſs by uſing the ſerum of blood inſtead of ſize;

To ſucceed, the following methods muſt be uſed:

1ſt. The butcher muſt be directed to receive the blood of one or more oxen in proper veſſels. When the blood is quite cold, that is to ſay three or four hours after it is drawn, the veſſels muſt be gently inclined, by which means, a clear, amber-coloured liquor will be decanted. This muſt be paſſed through a ſtrainer to ſeparate the fragments of the clot which have broken off and been mixed with it.

2d. Reduce to powder quick-lime, which has been ſprinkled with a little water to diminiſh the adheſion of its integrant parts. Paſs this powder through a ſieve, and depoſit it immediately in boxes or bottles well cloſed.

with ſlaked lime.

* Bibliothèque de Sonini, No. V.

3d. When

Manner of uſing it, &c.

3d. When theſe two materials are to be uſed, the ſerum is firſt poured into a wooden or earthen veſſel, and mixed with a ſufficient quantity of the lime, pulverized as directed above, taking care to preſerve the mixture of a proper fluidity to be eaſily ſpread with the bruſh over the ſurfaces to be covered.

4th. Too great a quantity of this paint muſt not be prepared at once, becauſe it thickens very faſt, and when it has acquired too much conſiſtence it cannot be uſed. This inconvenience may, however, be remedied by keeping the fluidity at the ſame point, by the addition of a ſufficient quantity of ſerum, which ſhould always be kept in a veſſel near that containing the paint, that it may be uſed when neceſſary.

Addition of colour.

5th. The colour ſo prepared, ſhould be uſed as quickly as poſſible.

General remarks.

6th. As the colour reſulting from the application of this preparation is always white, and as there are circumſtances in which a different colour is wiſhed for, this may be obtained, by adding a bolar earth of a red, black, green, or yellow ſpecies to the ſerum, at the ſame time as the lime is mixed. Even a beautiful blue may be obtained by uſing the blue glaſs prepared from oxide of cobalt, provided it be reduced to an impalpable powder.

7th. The ſtrength of the compoſition being neceſſarily leſſened by the addition of the bolar colouring matters, the ſame degree of ſolidity may be preſerved by adding to the ſerum uſed for diluting this compoſition, a few whites of eggs; but particular care muſt be taken not to put too many, otherwiſe the paint will be ſubject to ſcale off.

8th. This paint can only be applied on wood-work or coatings of plaſter, which have not been previouſly covered with oil-paint.

9th. As one layer will not be ſufficient, two or three may be laid on thoſe ſurfaces which are required to be well painted; but before laying on a ſecond coat, that which has been already applied muſt be perfectly dry.

10th. A beautiful poliſh may be given to this paint by friction, in the ſame manner as to other ſorts of paint; it need only be obſerved, that it is better to greaſe the cloths uſed for rubbing it, with clear whale oil, rather than with any other kind of oil.

11th. To

11th. To temper this paint whether white or coloured, it is neceffary that the ferum fhould be frefh and not have undergone any change, otherwife the paint will be of a bad quality and not durable.

The prefervation of the ferum, particularly in the fummer, requires much caution, becaufe this fluid has a great tendency to putrefy. It is therefore effential to keep it in a cool place, and to examine before ufing it, whether it has begun to emit a bad fmell; for, in that cafe, care muft be taken not to ufe it.

For the fame reafon the veffels in which the ferum is kept muft be carefully cleaned, and wafhed frequently with hot water, to remove the fpoilt portions of this fluid, with which the pores of the veffels might be impregnated.

M. *Carbonell* afferts that this paint is fo durable, that when prepared with good materials, it may be ufed to paint the walls of damp houfes, without fear of its coming off; an advantage certainly not poffeffed by diftemper painting. Very durable.

The fame author alfo afferts that he has made many experiments with this fame paint, and has obtained fatisfactory refults; and in all cafes fo conftant, that he doubts not, when it fhall be known, that it will be generally adopted. He inftances, among other examples, the ufe which is made of it at Barcelona, as well in the exterior of buildings as withinfide, and he has always obferved, not only that the fun, the air, moifture or drought, produce no change on it, but alfo that it is free from bad fmell; fo much fo, that places where it has been ufed may be inhabited without danger the very day of its application. May be ufed in out-door work.

One might be difpofed, at the firft glance, to believe that the new paint propofed by M. *Carbonell*, is nearly the fame with that of milk, defcribed fome years ago by Cit. *Cadet Devaux*. This latter paint may alfo have fucceeded; but on reflecting upon the effential difference which exifts between the ferum of the blood and that of milk, it will foon be perceived that if the milk paint is good, that of M. *Carbonell* muft be better. It is very different from the milk paint.

But experience muft decide in this refpect, and it is to be prefumed that a fhort time will make known which of the two methods deferves the preference.

The work in which M. *Carbonell* has given the details relative to the preparation and ufe of his paint with ferum, is dedicated

dedicated to the queen of Spain. This work has been printed in the Spanifh language: it would be very worthy of being tranflated into French, which would be the means of giving it greater publicity.

ACCOUNT OF NEW BOOKS.

North London canal.

Report of the intended North London Canal Navigation; with general Eftimate, &c. &c. By R. DODD, *Engineer.* 1802. *p.* 23 *Quarto.*

THE line of this intended canal is to communicate with the Thames near Bell-Wharf, and, paffing through Ratcliffe and Whitechapel, is to join a bafon near Hackney turnpike, from which the the main line is to extend to the river Lea near Waltham Abbey, Two collateral cuts are alfo to be continued from this bafon for the convenience of the eaftern and northern fuburbs of the city. From Waltham Abbey the river Lea is to be navigated as far as Bifhop's Stortford, at which place the intended line will again commence and be continued until it forms a junction with the Cam below Cambridge, From thence that river is navigable to the wafh or Lynn deeps.

The advantages to be derived from the propofed meafure are numerous, whether confidered as facilitating the conveyance of provifions to the capital; as furnifhing a fafer and cheaper communication from the wharfs and warehoufes at the eaftern extremity of the town, to the northern and weftern parts of it; as leffening very much the diftance and danger of water carriage between the metropolis and the whole of the eaft coaft; or, as affording the government a ready means of tranfporting troops and military ftores in the event of an invafion of that part of the Ifland.

The general advantages of Canals as well as thofe peculiar to the prefent ftatute are ftated with confiderable perfpicuity by Mr. Dodd in the prefent volume.

Defcription

Description of an improved Armillary Sphere exhibiting at one View the true Solar, or Newtonian System, agreeabty to that Order, Harmony, Beauty, and Variety observable in the Heavens; whereby the Science of Astronomy will be familiarly exemplified. By T. Pattrick, *Optician, and Manufacturer of* Adam's *and* Senex's *Globes,* No. 29, King Street, Covent-Garden, 1802. *pp.* 24. 12*mo.* **Pattrick on the armillary sphere,**

THE patent nautical angle, whereby a ship's departure, meridional difference of latitude, &c. are obtained from inspection, with the greatest accuracy, in plain sailing, oblique, current and mercator, with a sliding scale of meridional parts, graduated to four miles. By the same author, 1803, pp. 19. 12mo. **on the nautical angle.**

Theorie des Vents et des Ondes: A Theory of the Winds and Waves, by *M. de la Coudray.* 1 Vol. in 8vo. Price 3 Francs. Bernard, Paris, 1802.

Exposé des Temperatures: An Account of Temperatures, in which the various state of the atmosphere, and the influence of the air on the human frame, animals and plants, are treated on in the way of aphorism. Price sewed, 5 Francs. Barrois, Paris, 1802.

De L'Electricité Medicale: A Treatise on Medical Electricity, by Cit. Sigaud Lafond. Professor of Natural Philosophy and Chymistry, in the Central School of Chers. Associate of the National Institute, &c. 1 Vol. 8vo. 618 pages, with plates, 6 Francs sewed. Delaplace, Paris, 1802.

Nouveaux Traité sur la Construction et Invention des nouveaux Barometres, Thermometres, Hygrometres, Aereometres et autres Decouvertes de Physique Experimentales: A Treatise on the Construction and Invention of Barometers, Thermometers, Hygrometers, Areometers, and other Discoveries in Experimental Philosophy, by Assier Perricat the Elder, Engineer, &c.: with Meteorological Observations made on the Mountains by many learned Men, and by the Author himself, with comparative Tables. 1 Vol. 8vo. Price 2 Francs, 10 Cents. Paris, 1802.

Traité

New Books.

Traité d'Optique: A Treatife on Optics, a new Edition revifed and corrected, with Additions, particularly on the Production of the Images in Optical Inftruments, on the Achromatic Telefcope, and on the Iris, by many Pupils of the Polytechnic School, with all the Plates of the old Edition, and the Addition of many new Ones, by Lacaille. 1 Vol. 8vo. Price 3 Francs.

Nouvelles Experiences Galvaniques: New Galvanic Experiments by Nyften, Phyfician, 8vo. Price fewed, 2 Francs. 50 Cents. Levraults, Paris.

A
JOURNAL
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AND
THE ARTS.

JULY, 1803.

ARTICLE I.

On the Naturalization of Plants. By JOHN TEMPLETON, *A L. S. From the Irish Transactions,* Vol. VIII.

THE naturalization of plants is an object of ſuch importance, and a ſubject that is at preſent ſo little underſtood, that any attempt to extend our knowledge of it, however trifling, may ſtill tend towards improvement, and perhaps ſerve as a foundation, on which at ſome future period a more perfect ſtructure may be erected. Many experiments are yet wanting, much remains to be yet done, and, like other branches of knowledge, it will require the united efforts of numbers to bring it to perfection. Naturalization of plants little underſtood,

The ſame Almighty hand that formed the earth, has ſcattered in far diſtant regions vegetables which the neceſſity or luxury of man excites him to endeavour to accumulate about his home. And if we at the preſent time ſurvey the different nations of the earth, we will find that moſt of them have received great and important benefits by the introduction of foreign plants; and that there is no country, however numerous its collection of plants, but may yet receive conſiderable advantages by the naturalization of others. though highly beneficial to countries.

Botany, a ſcience which every one engaged in the ſtudy of will readily acknowledge to afford one of the pureſt of human Particularly to the botaniſt.

 pleaſures,

pleaſures, from the introduction of exotics derives its principal ſupport; and certainly whatever tends to facilitate this amiable ſtudy is truly deſerving of the attention of every philanthropic mind. As all botaniſts cannot have an opportunity of examining plants in their native ſoils at proper ſeaſons, it is therefore only by tranſplanting and cultivating, they can become acquainted with the productions of diſtant countries; and to cultivate them with ſucceſs we muſt derive our information principally from the plants themſelves: each has certainly a peculiar character, which were we truly acquainted with, thoſe tedious experiments with each newly acquired ſpecies, which now nearly exhauſt the patience of all lovers of plants, would be no longer neceſſary; gardening might then boaſt of being eſtabliſhed on ſcientific principles, and would then never adopt rules contrary to what nature dictates.

Nor will ſhe, ſcorning truth and taſte, devote
To ſtrange and alien ſoils, her ſeedling ſtems;
Fix the dark ſallow on the mountain's brow,
Or to the moſs-grown margin of the lake
Bid the dry pine deſcend. From nature's laws
She draws her own: nature and ſhe are one.

MASON'S ENGLISH GARDEN, B. 111, line 226.

By our preſent imperfect knowledge of the phyſiology of plants, we are neceſſitated to accept of every aſſiſtance within our reach: and plants being ſo immediately connected with every modification of the atmoſphere, meteorology, which has hitherto been conſidered as an object of curioſity, is a ſource from which we may derive much uſeful information.

The mean heats of different climates conſiderably govern their vegetation,

Heat being found to increaſe or decreaſe nearly in a regular progreſſion, according to the degrees of latitude, if the latitude of the place where a plant is found be known, by conſulting Mr. Kirwan's table of the mean annual temperature of different latitudes *, we may find whether the temperature nearly correſponds with our own. Or, ſuppoſing the mean annual temperature of Dublin, lat. 52°, equal 50 of temperature, by adding one for every degree of latitude ſouthward, and ſubtracting one for every degree of latitude northward, we have the temperature correctly enough for our purpoſe. For theſe

* See an eſtimate of the temperature of different latitudes by R. Kirwan, Eſq; page 17.

calculations need not be carried to the greateſt degree of correctneſs, as we know that, if we except a few, plants have a conſiderable range of latitude; thoſe which cannot bear froſt, being found to extend from the northern to the ſouthern verge of the torrid zone, and many of thoſe which grow on the ſouthern limits of the temperate, to approach the borders of the frozen zone. Thus of the Lapland plants near three hundred are found in the environs of Paris *, many of them much farther ſouth, and ſome, as the water lillies *(nymphæa)*, ſundew *(droſera)*, arrow-head *(ſagittaria)*, &c. even natives of India †.

but with conſiderable latitude.

In the latitude 44° on the European, and 34° on the American continent ‡, it is not unuſual for water to be frozen in January; and as ſome ſeaſons are much more ſevere than others, plants growing conſiderably farther to the ſouthward would be liable to ſuffer by cold in ſuch ſeaſons, if nature had not provided a remedy by their manner of growing, which enables them to reſiſt the cold of ſuch rigorous ſeaſons; and on this account many of them will be found to thrive, when tranſplanted nine or ten degrees further north than their native ſtations.

Plants do greatly accommodate themſelves to the climate.

From ancient authors it appears that Italy formerly experienced the ſame degree of cold as the American continent under the ſame parallel of latitude does at preſent. Therefore it is highly probable, that Italian plants not introduced into Italy ſince that time, might in a ſeries of years be changed from their now tender to their once hardy ſtate.

Inſtances in the ancient and preſent climate of Italy.

But in conducting ſuch an experiment as the naturalization of plants from a ſouthern to a northern climate, ſo many minute circumſtances require our attention, that few people have either time or patience to reach the wiſhed for goal.

But a careful attention to the characters which the plants themſelves preſent, will enable us to proceed with more certainty, and hopes of having our endeavours crowned with ſucceſs.

By the appearance of the roots and leaves we may nearly determine in what kind of ſoil the plant is moſt likely to thrive. Robuſt roots and fleſhy or rigid leaves require a dry ſoil, ac-

In naturalizing plants particular attention muſt be paid to the ſoil.

* See Flora Lapponica & Thuillier's Flore des Environs de Paris.

† See Hunters Evlyns Sylva, p. 552.

‡ See Kirwan on the Temperature of different Latitudes, p. 50.

Inftructions.

cording to their thicknefs; ftiff clay or fandy loam, as beans, peach, and apple trees; robuft fpongy roots which have a tendency to mat near the furface with thin leaves, as the alder *(betula alnus)*, willows *(falix)*, require a fomewhat ftiff foil with moifture; many of the falix genus will not grow with their accuftomed vigour, in a light turfy or peat mold foil, for want of the neceffary refiftance to the roots, although fuitable in refpect to moifture. Slender, hard and wiry roots, as thofe of the pine, ciftus, &c. require dry, fandy, or gravelly foils. And extremely fine and hairlike roots, as thofe of erica, halmia, rhododendron, &c. muft have a foil whofe particles will not impede the fhooting of their tender fibres, and with a fmall but regular degree of moifture, that the roots, which by their form cannot refift the flighteft drought, may not be deftroyed. Plants in a warm climate perfpire more than in a cold one; fo in a warm they require much, and in a cold one little moifture. Therefore, when tranfplanted from a warm to a cold climate, they fhould have a dryer foil, and from a colder to a warmer, a moifter one, than their native ftation.

In tranfplantation to a cold climate the foil muft be dryer; and the contrary.

In the firft cafe, not being able to perfpire the fuperabundant moifture, they will be rotted; and in the laft, not having moifture fufficient to fupply the lofs by perfpiration, the growth will be flow, difeafe and death will follow, unlefs they receive a timely fupply of moifture: by the red or yellow colour of the leaves we may difcern the approach of the firft evil, and by the ftunted growth, and fmall curled leaves, that of the laft. A large quantity of pure circulating fluid feldom injures plants, but ftagnant water is certain deftruction to almoft every vegetable.

Local fibration or expofure is of great confequence.

After having determined the moft fuitable foil, we muft afterwards ftrive to give each plant a proper fituation. It is well known, that plants from a fhady will not thrive well in an open, nor plants from an open in a fhady fituation. But the neceffity of a natural fituation is by no plant more evidently illuftrated than by the common myrtle *(myrtus communis)*, Even at Glenarm, in the latitude 54° 56′ N. it grows with great luxuriance contiguous to the fea, and braves our coldeft winters; yet all attempts to naturalize it in an inland fituation, feveral degrees farther fouth, and in a much more genial climate, have hitherto proved unfuccefsful. The olive tree cannot

cannot poffibly be cultivated in the interior of Afia or America, though the latitude be in other refpects favourable, nor is it fruitful when excluded from the fea breezes *.

The myrtle grows well near the fea, becaufe the temperature varies lefs. Other remarks.

The caufe of this may be that near the fea the temperature is more regular than within land, and fudden changes are perhaps unfavourable to evergreens: for we always find thofe with broad leaves grow beft in the fhade, and thofe with narrow leaves on elevated places, in both of which fituations the temperature is more regular than in open expofures or confined vallies. And we may often obferve plants growing on a fomewhat elevated fituation, if fheltered from ftrong winds, lefs hurt in a fevere winter, than others in low warm and fheltered places. For vapour being raifed in fuch places during the day, produces a greater degree of cold by condenfation and evaporation in the night, than is experienced in other places where the coldnefs of the air prevented the rife of vapour during the day. From every obfervation it appears, that thofe plants which have the leaft fap in winter, or whofe fap is of a refinous or oily nature, fuffer leaft from cold, and that the principal caufe of deftruction is the veffels being burft by the freezing of the fap. The hoar-froft, which is always moft abundant in vales, tends in a great degree to promote this; for being changed into water, part only of this water is evaporated during the day, the reft remains to be converted into ice by the cold of the enfuing night. This icy covering encreafes the cold, till the vital principle †, and refiftance given by the formation of the bark to the entrance of cold, are overcome, the fap frozen, and at the fame time the veffels burft by the expanfive force of freezing. This gives the reafon why plants in a fituation where the fun does not fhine on them to thaw the hoar-froft, fuffer leaft in fevere feafons; and that plants removed in autumn, unlefs the fhoots are completely hardened, will be more liable to be injured by froft than thofe of the fame fpecies, the defcent or fixation of whofe fap has received no check by tranfplanting. Miller remarks, that thofe plants which were removed in the autumn of 1739, were

* See Saint Pierre's Studies of Nature, tranflated by Hunter, Vol. I. page 607. Dublin edition.

† See Smith's Tracts relating to Natural Hiftory, page 177, and Philofophical Tranfactions for 1788.

moftly

moftly killed by the cold of the enfuing winter, while many of the fame fpecies efcaped uninjured: and the fame may be always obferved after every fevere winter *.

Sunfhine and fhade.

Few deciduous fhrubs agree with fhade; their natural place is the funny outfkirts of the foreft; and when otherwife fituated, long and flender branches, with large thin leaves, fhew their unhealthy ftate. From thefe the climbing plants are eafily diftinguifhed by their tendency to contortion, or fhooting forth roots or tendrils. To the deciduous climbers a flight fhade is not hurtful, as it is only there they can find the neceffary fupport; but in the deep receffes of the foreft, the evergreen climbers will fpread around their tangling branches, and thrive with wild luxuriance,

> Where fcarce a funbeam wanders through the gloom.
>
> THOMSON'S SEASONS.

never appearing, if the foil is fufficiently moift in fummer, to be hurt by the thickeft fhade of deciduous trees.

Shade defends plants from froft.

The fhade is alfo the natural fituation for young plants. By the parental fhade they are protected from the drought of fummer, and the cold of winter. The more a plant is fhaded in winter, and the nearer it is to any large body, the lefs danger it will be in of fuffering from froft. For when a plant or water is fo fituated as to be overtopped by trees, a great part of the hoar or frofty particles, which would fall on it, is intercepted. Under trees we may often obferve water unfrozen, and plants unhurt by the feverity of cold, and many retaining their leaves; when water at a fmall diftance is frozen, and plants of the fame fpecies, but unfhaded, lofe their leaves and fuffer confiderably. As large bodies are not eafily cooled, the cold is in fome degree mitigated by the ftems of large trees. That this is the cafe may be perceived, the twigs and fmaller branches being covered with hoarfroft, when the trunk and larger branches remain uncovered.

* The following experiments may throw fome light upon the caufe of plants remaining unfrozen, when the furrounding water is frozen. Water enclofed in fealed glafs globules remains unfrozen, till the thermometer defcends to twenty-four; unfealed ones freeze and burft immediately on being cooled down to freezing water. Oil enclofed in the fame kind of globules continued unexpanded, and confequently the globules unbroken, when placed in a mixture of fnow and fal ammoniac, and cooled below o.

Dr,

Remarkable inſtances of the prevalence of froſt on inſulated bodies.

Dr. Wilſon of Glaſgow obſerved, that when a great degree of cold prevailed, paliſades extending outward from a houſe, and alſo from a large pillar, were covered with hoarfroſt, in the moſt regular manner, according to their proximity or diſtance from the houſe or pillar, thoſe next the houſe or pillar remaining free from hoarfroſt, while the more diſtant ones were entirely covered *. This accounts for the fig-tree ſhoots, mentioned by Miller, being killed when growing out from the wall, at the ſame time when the other ſhoots cloſe nailed to it eſcaped unhurt †. From this circumſtance, moſt people have affixed thoſe plants which they wiſh to naturalize to the climate againſt walls. But when put to a wall, care ſhould be taken that they are ſheltered from ſtrong winds, which generally injure the leaves and young ſhoots, thereby deſtroying the plant if it is not vigorous. On this account the ſtems of large trees are preferable for climbing plants, and there they muſt always be more admired as appearing more natural.

But inſtead of affixing to walls thoſe plants which require no ſupport, we might cultivate them in pots or boxes, which may be placed in their proper expoſure during the ſummer, and, until their hardineſs is determined, removed under the fir or other trees in winter, the thickneſs of whoſe ſhade ought to be proportioned to the apparent tenderneſs of the plant.

Theſe pots or boxes ſhould be always ſunk in the earth, and in winter the ſurface covered with moſs. The drier the ground the better, for ſunken pots are liable to be too damp.

Treatment of ericas and other plants.

The beſt manner of treating ericas is to place them in a proper ſituation in the ſpring, and on the approach of ſevere weather to fix branches of ſpruce-fir about them, augmenting the covering as the cold encreaſes. But as the cold ſeldom becomes ſuddenly ſevere, and a ſlight froſt does them little injury, the erica tubliflora, one of the moſt tender, bearing about 29° of Fahrenheit's thermometer; it is beſt not to begin covering too ſoon, leſt, as they are plants that require a very ſmall degree of heat, they ſhould be made to ſhoot, in which caſe the ſlighteſt froſt will perhaps deſtroy them. To protect herbaceous plants from froſt, moſs is the moſt proper covering, by remaining alive through the winter; even after being pulled up, it is not liable to heating and putrefaction, as all dead ve-

* Philoſophical Tranſactions, Vol. lxx. p. 471, 2.
† Miller's Dictionary, Article Ficus.

getable

getable ſubſtances are, by which they impart to the plant heat and moiſture (the two principal agents which cauſe vegetation) thereby putting the vegetating powers in action, and filling the plant with ſap, at an improper ſeaſon. In our culture of annuals our only care is directed to placing them in proper ſoils and expoſures. For, that no region of the earth ſhould remain uninhabited, with a liberal hand have the annual plants been diſtributed; from theſe do men and animals derive their principal ſupport, and of all the vegetable kingdom, they are beſt adapted for naturalization. By bringing their ſeed to perfection in a ſingle ſeaſon, they are capable of cultivation in a greater variety of climates than any other vegetables. And the ſeed, being equally undeſtroyed by natural heat and cold, lies dormant, till genial weather calls forth its latent powers, and urges it to vegetation, whether among the frozen ſnows of Siberia, or the burning ſands of Africa. The *reſeda odorata* (mignionette), a native of Egypt, and *helianthus annuus* (ſunflower), of Mexico and Peru, ripen their ſeed, and are thereby perpetuated in our northern latitudes. St. Pierre * ſays, the peaſants of Finland cultivate tobacco *(nicotiana tabacum)*, with ſucceſs, beyond the ſixty-firſt degree of latitude; and that barley ſucceeds in the very boſom of the north. Amidſt the rocks of Finland he ſaw crops of this grain as beautiful as ever the plains of Paleſtine produced †.

Criterions by which hardy exotics may be diſtinguiſhed.

When we endeavour to naturalize plants, that we may diſtinguiſh thoſe which offer the faireſt proſpect of ſucceſs, a compariſon of the exotics with the natives of the ſoil will be our ſureſt guide. Thus we find, that throughout the froſty regions of the north, the trees, ſhrubs, bulbous and perrennials, complete their ſhoots, and, before the cold of the winter commences, encloſe in hybernacule or ſcaly buds, the embryo for the coming year. And there is every reaſon to believe that all exotics will ceaſe growing, and form theſe buds or hybernacule in the open air during the courſe of our ſummer, will not ſuffer from the ſeverity of our winter. In the hot-houſe many plants complete their ſhoots that would not probably do ſo in the open air, the heat not being ſufficient to cauſe them to grow with the vigour neceſſary for their completion before winter.

* See St. Pierre's Studies of Nature, tranſlated by Hunter, Dublin edition, Vol. I. page 604.

† See the ſame Work, page 667.

Nevertheleſs

Neverthelefs many of thefe, if not all, might be brought, by enuring them to the open air, to bear our climate. The camelia japonica, thea viridis, and calycanthus præcox, which were formerly kept in the hot-houfe, then in the green-houfe, are now fufficiently naturalized to grow in the open air, and are as little injured with the cold of our winters as either the common or Portugal laurels.

Exceptions to the rule.

Some exceptions to this obfervation feem to prefent themfelves. The *robinia pfeudo acacia* (two-thorned acacia) does not form external hybernacule, nor complete its fhoots, yet grows well in our climate; it however, when the froft comes on early, lofes a great part of its fummer fhoots. Several fpecies of the ciftus, that ceafe growing on the approach of winter, but form no hybernacule, live through our mild winters, but fuffer greatly in fevere frofts. And others, as the laurufłinus *(viburnium tinus)* continue to fhoot and flower, unlefs the froft is fevere throughout the winter, fuftained by their vitality, or that principle whofe exiftence preferves plants unhurt by cold before flowering, but which ceafes to exift when the parts of fructification have performed their office *.

On the laurustinus, buddlea globofa and fufchia coccinea.

The laurufłinus is one of thofe plants that were introduced to Ireland before green-houfes were known, confequently planted in the open ground, and experience fhews that it is feldom hurt by froft. By it we find that fome plants, which to appearance are not fitted for our climate, do yet outlive our winters; and that, without a knowledge of their native ftations, we may fometimes fuppofe plants to be tender which are really hardy: thus the laurufłinus is unhurt by froft in Ireland until the cold exceeds that of its own climate. The buddlea globofa and fufchia coccinea are other inftances of plants, that without a knowledge of their native climate, Chili, we would not fuppofe capable of being naturalized to ours. Yet is the buddlea feldom injured by our cold, and the fufchia, although killed to the ground by the winter's cold, fends forth abundance of fhoots which attain the height of three feet in fummer, and are decorated with its elegant flowers, which are larger and much more brilliant than ever they are when confined in a houfe:

* See Smith's Tracts, page 177, and Philofophical Tranfactions for 1788.

And

The plants of elevated tropical regions would endure the cold of the higher latitudes.

And there is a little doubt but many plants of Chili, and even thofe which grow within the tropics, when found near the elevation of perpetual froft, would bear the cold of Spitzbergen; for on the tops of mountains are found the plants of the plains of more northern latitudes. Thus is the falix herbacea of Lapland and Spitzbergen found on the tops of Mourne mountains at about the elevation of 2,500 feet. On the Serra of Madeira, latitude 32°, 38′, and elevated 5,162 feet, is found the erica arborea, of the neighbourhood of Genoa, latitude 44°, 25′ *. Therefore as the temperature which prevails at the elevation of 5,162 feet, in latitude 32°, is found nearly to correfpond with that of 51° north: the erica arborea, which grows at that elevation in latitude 32°, will find a climate fuited to its nature in latitude 51° †. But as the before mentioned plants have a confiderable range of latitude, it may be cultivated farther north when the foil and fituation are favourable. At James Holmes's, Efq. on the eaftern fhore of Carrickfergus Bay, four miles north of Belfaft, there is a plant in the greateft vigour at the prefent time (July 1799) which has now ftood uninjured three as fevere winters as Ireland ever experienced, viz. 1794, 5, 1797, 8, and 1798, 9 ‡.

The fituation is however favourable, being againft a weftern wall facing the fea, and well fheltered by diftant trees from ftrong winds. And in the neighbourhood of the fea I have little doubt but it would grow ftill farther north.

The fea air is affirmed to be no obftacle to plantations; but that the open expofure is alone pernicious.

The fea air has generally been reckoned a powerful obftacle to have plantations on its fhores. But many obfervations have convinced me that it is the wind alone which prevents the growth of trees on the fhores of the fea. Aud that on a large plain, where the winds are unimpeded in their courfe, the fame difficulty of raifing plantations as on the margin of the ocean will be experienced.

* See Sketch of a Tour on the Continent, by J. E. Smith' M.D. F.R.S. &c. page 200, Vol. I.

† On dividing 15,577, the height of perpetual froft at the equator, by the difference of the temperature above and below, it is found that every 299 feet of elevation leffen heat 1°, and on dividing 5,162 feet by 299, we have 17°, which fubtracted from 69 mean annual temperature of latitude 32°, give 52, for the Serra of Madeira, correfponding with the latitude 51° (I.)

‡ See Kirwan's table of mean annual temperatures.

In

In Foſter's account of Cook's ſecond voyage, it is mentioned, that the trees on New Zealand were growing ſo cloſe to the edge of the water, that the ſhip's maſts were entangled among their branches; and in particular ſituations the ſame proximity of trees to the ſea might be obſerved in various latitudes. At Fairhead, the moſt northerly extremity of Ireland, and expoſed to the fury of the northern ocean, the *ſorbus aucuparia* (mountain aſh), *betula alba* (birch), *quercus robur* (oak), with other indigenous trees, grow luxuriantly within 15 or 20 yards of high-water-mark. The reaſon of this appears to be that they grow upon the lower part of very high land, which cauſes an eddy to be formed about them when the wind blows from the ſea; and by the ſame high land they are protected from the ſouth and ſouthweſt winds.

Inſtances of trees growing cloſe to the ſea where ſheltered.

On the top of the rocks the wind rages with the greateſt fury, even the graſs ſeeming blighted, whereas below the rocks every plant appears in a thriving ſtate, and ſome houſes ſituated on the lower part never have their thatched roofs diſturbed by the ſtorms. In every other part along the coaſt where land is of the ſame form it is covered with thriving wood, but where the land is nearly level for a length of way inland no wood appears, and every hedge is ſeen never to riſe higher than the top of the bank which protects it from the wind. Therefore in order to plant near the ſea on a low ſhore, it is neceſſary to commence the plantations a conſiderable way inland, and to allow the young trees to have others ſeveral feet taller than themſelves behind them: theſe will have the ſame effect as high land, for by means of the oppoſition offered by innumerable ſtems and branches the force of the wind will be greatly leſſened; as we may find by ſtanding on the windward ſide of a thick wood during a ſtorm, where, if the trees are lofty, the wind is much leſs violent than on an open plain. In water the effect of this kind of oppoſition is viſible, for if into the bed of a ſwift ſtream we drive a number of ſtakes, the water, although it continues to flow, yet has its velocity diminiſhed conſiderably.

As plantations thrive near the ſea when ſheltered, it is proper to begin to plant inland, in order that the firſt trees may protect thoſe near the coaſt;

Our firſt plantations in an expoſed place ought always to be of ſuch trees as are natives of mountains for theſe are fitted by nature to bear the rude blaſts of winter, and by the ſtiffneſs of their leaves, or flexibility of their footſtalks, to re- main

and the firſt plantations ought to be natives of mountains

main uninjured by a fummer ftorm. Of the firft, we have the various race of pines; of the laft, the birch, the afpen and the mountain afh *.

Thus by a careful infpection of the operations of nature, is the hand of man enabled to collect the productions of diftant countries around his home, cover the arid heath with waving green, and make the lonely wildernefs affume a pleafing gloom.

II.

A Method of remedying certain Inconveniences arifing from the Inequality of Heat in large Diftillation. By SIR A. A. EDELERAUTZ †.

In diftillation the operations are 1. to evaporate, and 2. to condenfe.

IT is generally known that every diftillation confifts of two principal operations; the converfion of the matter under diftillation into fteam by heat, and the condenfation of that fteam by cold. In order to the fpeedy accomplifhment of thefe two objects, and without any unneceffary expence of fuel, a perfect equilibrium muft be eftablifhed between the evaporating heat and the condenfing cold; that is to fay, fuppofing this laft to be conftant, as it may be practically made, (a given quantity of water at a fixed temperature paffing through the cooler in a given time.) The heat muft then be fo regulated that the quantity of fteam produced fhall be neither more or lefs than can be condenfed in the fame time, by the cold applied to it. A want of attention to this circumftance produces the two following inconveniences, particularly in the diftillation of fpirituous liquors.

The heat muft be adjufted to the cooling power.

Strong heat waftes fuel and diffipates part of the vapor.

1ft. If the fire be too brifk, a confiderable quantity of the confined fteam will pafs from the worm into the open air, occafioning a lofs of the matter under diftillation, and a wafte of fuel.

* Among the rocks of Agnew's Hill in the county of Antrim, I found the populus tremula (afpen tree) growing luxuriantly on the eaftern face, at about the elevation of 1,450. And on the top of Slemifh, the forbus aucuparia (mountain afh) expofed to every ftorm at the elevation of 1,398 feet.

† Annales de Chimie, XLV. 297.

2nd.

2nd. If the fire be fuffered to flacken too much, the condenfation produces a vacuum in the worm and the alembic, which not being fupplied in the fame proportion by frefh fteam, occafions the outer air to enter, which renders the evaporation and condenfation more difficult, and when it is, at laft, driven out, it takes with it a portion of the fteam, thus occafioning a lofs of the matter under diftillation, as well as a lofs of time.

Low heat caufes the outer air to enter, which when expelled carries out vapor, &c.

In order to remedy thefe defects, and at the fame time to furnifh a fimple method of regiftering the actual heat, I have thought of the following inftrument, which is applicable to any diftilling apparatus, and is nothing more than an application of known principles of theory to actual practice.

Inftruments for remedying thefe defects.

a, b, c, d, Fig. 1. is a bent tube of copper or glafs in feparate pieces, with a bulb at *b;* the upper end of the tube *a* may be attached to the worm by means of a fcrew. The length *b c, c d* is of four feet, and the capacity of the bulb *b,* is a little larger than the whole capacity of the tube *b c d.* The diftillation being begun, the condenfed fteam will pafs by *a* and the bulb *b,* into the tube *b c d;* and when its two arms are full, the liquor will run out at *d,* into the veffel intended to receive it. Thefe two arms, therefore, remain full during the whole of the diftillation, and in this confifts the remedy to the inconvenience above-mentioned; it is evident that if the fire becomes too quick, the uncondenfed fteam cannot force a paffage to the outer air, in order to diffipate itfelf, till it has driven out all the liquor contained in the tube *b c,* and has overcome the preffure of a column equal in height to *c d.* Again, the outer air cannot enter to fill up the vacuum produced by the flownefs of the fire, but by driving back the column *d c,* and overcoming the preffure of the fame height. Now this column being four feet, gives range and time fufficient for the operator to regulate the fire accordingly. If the tube *b c d* were made of glafs, we fhould only have to obferve the level of the liquor in the two arms, the fall in *b c* would indicate that the heat fhould be diminifhed, and in *c d,* that it fhould be increafed: but tubes of this length being fubject to accidents, I prefer attaching to *c* a fmall regulator of glafs, *e f e,* the two arms of which, *e f,* each of three inches long, contain mercury, and which rifing in them alternately, will indicate the ftate of the heat and the fteam with fufficient accuracy. This regulator may be placed in a bottle or flafk, which will protect

An inverted fyphon of condenfed fluid, and a mercurial gage which allows of fome latitude of internal or external preffure, without communication from without.

protect it from injury. Between it and the worm is a cock *g*, which at firft communicates with the outer air, but after having produced a ftrong heat, the fteam is feen efcaping at *g*; by turning it a communication is opened between the worm and the regulator, which then begins its operations. The bulb *b* prevents the liquor preffed by the outer air from rifing into *e f e*, and the alembic. It is fuperfluous to add, that, of whatever form the capital may be, it fhould be well luted, in order to prevent the accefs of the outer air.

III.

Account of a Time-Piece for regiftering the Attendance of Watchmen, and for fimilar Purpofes; conftructed by Meffrs. BOULTON *and* WATT, *for the Right Honourable the* MARQUIS OF EXETER. *In a Letter from his Lordfhip.*

To Mr. NICHOLSON.

SIR,

HAVING feen Mr. Day's account of his watchman's regulator in your Journal, dated June 1, 1803, I think it may be proper to inform you that a clock for a fimilar purpofe has been invented by Meffrs. Boulton and Watt, Birmingham, which cofts no more than thirty fhillings. I have had two of them above four years. They go eight days, and have a face like a clock, but do not ftrike. The dial goes round, and the hour finger is fixed; round the edge of the dial are moveable iron pins, correfponding with the quarters in each hour. A fmall hammer placed behind the hour finger, when moved downwards, pufhes into the dial, one of the pins which happen to be under it at the time, which pin remains fo abafed until the dial nearly returns to the fame place, when by an inclofed plane the pin is raifed up into its firft pofition. This gives time to have the machine examined in the morning, to fee how many pins have been ftruck, and at what time they were pufhed downwards. The hammer is moved by the pulling of a chain with a handle, like houfe-door bells, which, by cranks and wires is attached to it. I have one in my library, the handle is out of doors. The other machine is placed n a build-

ing

ing at the other end of my premifes. I have always two watchmen every night, and they go the round every half hour.

I am,

SIR,

Your obedient humble fervant,

EXETER.

Burghles, June 9, 1803.

IV.

Some Account of the Poifonous and Injurious Honey of North America. By BENJAMIN SMITH BARTON, M. D.*

IN the year 1785, I had an opportunity of obferving fome of the difagreeable effects of our wild honey upon feveral perfons who had eaten of it, in the weftern parts of Pennfylvania, near the river Ohio. From thefe effects I was perfuaded, that a fubftance which is generally confidered as entirely innocent, is capable of doing much injury to the conftitution. I was, therefore, induced to pay fome attention to the fubject. The refult of my inquiries I now communicate to the Philofophical Society.

Honey is fometimes of fuch a nature as to injure the conftitution.

It is not neceffary to make any remarks on the fabric of honey. It may be fufficient to obferve, that the honey will always partake, in a greater or leffer degree, of the fmell, the tafte, and general properties, of the flowers from which it is obtained. This obvious fact fhould have folicited more of the attention of thofe whofe employment it is to raife large numbers of bees, for the purpofe of obtaining the valuable product of thefe little infects. But, in this country at leaft, hardly any attention has been paid to the fubject. Perhaps, the following loofe hints, by pointing out fome of the fources from which an ill-flavoured or pernicious honey is obtained, may be of fome fervice to the new or remote fettlers of our country.

It always partakes of the nature of the flowers whence it is gathered.

I muft obferve, that in thefe hints I do not mean to include among the difagreeable confequences of the eating of honey, the occafional effect of its purging: for although, as I fhall prefently obferve, a purging is one of the common effects of the poifonous honey, yet the moft innocent honey will often

Common honey.

* American Tranfactions, V. 51.

induce

induce the ſame ſtate of the body, when it is eaten in large quantities, or when it meets with an irritable ſtate of the bowels.

Symptoms produced by deleterious honey.

The honey which I call deleterious or poiſonous honey, produces, as far as I have learned, the following ſymptoms, or effects: viz. in the beginning, a dimneſs of ſight or vertigo, ſucceeded by a delirium *, which is ſometimes mild and pleaſant, and ſometimes ferocious; ebriety, pain in the ſtomach and inteſtines, convulſions, profuſe perſpiration, foaming at the mouth, vomiting, and purging; and, in a few inſtances, death. In ſome perſons, a vomiting is the firſt effect of the poiſon. When this is the caſe, it is probable, that the perſons ſuffer much leſs from the honey than when no vomiting is induced. Sometimes, the honey has been obſerved to produce a temporary palſy of the limbs; an effect which I have remarked, in animals that have eaten of one of thoſe very vegetables † from whoſe flowers the bees obtain a pernicious honey.

They are ſeldom fatal.

Death is very ſeldom the conſequence of the eating of this kind of honey ‡. The violent impreſſion which it makes upon the ſtomach and inteſtines often induces an early vomiting or purging, which are both favourable to the ſpeedy recovery of the ſufferer. The fever which it excites is frequently relieved, in a ſhort time, by the profuſe perſpiration, and perhaps by the foaming at the mouth. I may add, that as the human conſtitution reſiſts, to an aſtoniſhing degree, the effects of the narcotick and other poiſonous vegetables that are beſt known to us, ſo we need not wonder, that it alſo reſiſts the effects of the deleterious honey, which is procured from ſuch vegetables.

* An intelligent friend of mine related to me the caſe of a perſon who, for a ſhort time, was ſeverely affected from the eating of wild honey, in Virginia. He imagined that a perſon ſeized him rudely by one arm, and then by the other. After this, he fell into convulſions, from which, however, he recovered, in about an hour. It was imagined that this honey was obtained from a kind of poiſonous muſhroom.

† The Kalmia latifolia.

‡ We ſhall afterwards ſee, that not one of Xenophon's men died from the deleterious honey which they had eaten, in large quantities, on the ſhores of the Euxine-Sea.

It

Respecting the external signs of unwholesome or poisonous honey.

It deserves to be mentioned, that the honey which is formed by two different hives of bees in the same tree, or at a little distance from each other, often possesses the most opposite properties. Nay, the honey from the same individual comb is sometimes not less different in taste, in colour, and in its effects. Thus one stratum or portion of it may be eaten without the least inconvenience, whilst that which is immediately adjacent to it shall occasion the several effects which I have just enumerated.

I have taken some pains to learn what are the signs by which the deleterious honey may, at first view, be distinguished from innocent honey. I am informed that there is no difficulty in the matter.

The poisonous honey is said, by some, to be of a crimson-colour; by others, it is said to be of a reddish-brown colour, and of a thicker consistence than common innocent honey.

These are the signs by which, I am told, the most experienced hunters, in the southern parts of North-America, are enabled to distinguish pernicious from innocent honey.

On a subject such as this, I feel every disposition to pay a good deal of deference to the experience of an American hunter. Even philosophers may obtain much useful information from hunters, however wandering their life, however rude their manners. It is in the power of our hunters to enrich natural history with many important facts. But we ought not, I presume, to confide implicitly in every thing they tell us.

The external signs are not very conclusive.

I have good reasons for doubting whether the signs which I have mentioned will enable us, in every instance, to determine whether honey be poisonous or innocent.

The honey of the bee, undoubtedly sometimes partakes of the colour of the flowers from which it is gathered. The bees gather honey from many flowers of a crimson colour, and from many flowers whose colour is a reddish brown. In these cases, it is probable that the honey will sometimes borrow, in some degree, the colour of the flowers. Yet there are many crimson-coloured and reddish-brown coloured flowers that are perfectly innocent. The honey obtained from them will, I presume, be innocent also. Mr. Bruce says he was surprised to see, at Dixan, in Abyssinia, "the honey red like blood, and nothing," he remarks, "can have

Red honey.

an appearance more difgufting than this, when mixed with melted butter *." Nothing is faid, by this author, that can lead us to fuppofe that the Dixan honey was poifonous. From the manner in which it is mentioned, it is pretty evident that it was not poifonous. Linnæus informs, us that in Sweden the honey, in the autumn, is principally gathered from the flowers of the erica, or heath, and that this honey is of a fomewhat reddifh colour; and accordingly, he obferves, thofe provinces of the country that are deftitute of the heath, fuch

White honey.

as the province of Oelandia, furnifh a white honey †. The great naturalift fays nothing concerning the properties of the heath-honey. However, we may prefume, when we recollect the minute accuracy of Linnæus, that this honey did not poffefs any dangerous properties, otherwife he would have noted the circumftance. Whilft I refided in Edinburgh, I had the honey from the Highlands frequently brought to my table. I often remarked that this honey had a dirty brownifh colour, and I was told that it was chiefly procured from the different fpecies of erica, perhaps principally from the "blooming hather ‡," which abound in the Highlands. I never heard the people in Edinburgh, although they confume large quantities of this honey, complain that it poffeffes any noxious property. If it were actively poifonous, or injurious, the quality would have been, long fince, obferved. I well remember, however, that, for two years that I ufed it, it almoft always rendered me drowfy. Sometimes, indeed, it compofed me to fleep as effectually as a moderate dofe of laudanum would have done. A foreigner, who had not been accuftomed to eat anodyne honey, was better capable of remarking the effect which I have mentioned than the natives, who have been in the habit of ufing it from their infancy. I do not find that this fingular property of the Scots honey has been noticed by any writer §. I have, therefore, related it,

* Travels to difcover the fource of the Nile. Vol. V. or Appendix, p. 151. Quarto edition.

† Fauna Suecica.

‡ Burns.

§ Dr. Withering fays bees extract a great deal of honey from the flowers of the erica vulgaris, or common heath, and he remarks that "where heath abounds, the honey has a reddifh caft." A botanical arrangement of Britifh plants, &c. Vol. 1ft.

though

though it rather oppofes any objection to the figns employed by our hunters to diftinguifh poifonous from innocent honey. But he who is ftudious of truth, fhould relate ufeful facts as they are, without regarding what is their connection with a favourite fyftem, or opinion.

The learned Jofeph Acofta fpeaks of a grey-coloured honey comb which he faw in the province of Charcas, in South-America. The honey of this comb, he fays, is "fharp and black." He fays nothing farther of its properties *.

The poifonous wild honey is fcarcely to be diftinguifhed but by cautious trial.

An ingenious friend of mine †, to whom the public are indebted for a variety of valuable information concerning the natural productions of various parts of North-America, informs me, that, in the Carolinas and Floridas, the poifonous honey is fo fimilar, in colour, tafte, and odour, to the common, or innocent honey, that the former cannot be diftinguifhed from the latter. It is owing, he fays, to this circumftance, that fo many accidents daily happen from the ufe of the wild honey. He was informed, that it is experience alone which enables the hunters and others to determine, whether the honey which they find in the woods be poifonous or innocent. They have obferved that the injurious effects manifeft themfelves in a fhort time after the honey is taken into the ftomach. They are accuftomed, therefore, to eat a fmall quantity, before they venture to fatisfy their appetite. Should this produce *any* difagreeable effects, they do not think it prudent to continue the ufe of it. But, if in a fhort time, it fhould occafion no inconvenience, they think they may, with perfect fafety, indulge their appetite to the full.

Said to be rendered mild by boiling and ftraining.

I have been informed that the poifonous honey, by boiling and by ftraining, may be rendered as innocent as any honey whatever. It is, likewife, faid, that by long keeping it becomes harmlefs.

The honey of which I am treating is poifonous to dogs, as well as to men.

Remedies.

Hitherto, I have not been able to obtain any certain information concerning the means to be purfued in the treatment of perfons labouring under the effects of the poifonous

* The Naturall and Morall Hiftorie of the Eaft and Weft Indies, &c. p. 303.

† Mr. William Bartram.

honey. It is ſaid that the Indians, and ſome of the Whites, uſe cold bathing with advantage. It is probable that this practice has been uſeful. As the effects produced by this honey are ſo ſimilar to thoſe produced by ſeveral narcotic vegetables that are well known to us, ſuch as opium, henbane *, thorn-apple †, &c. it is probable that the ſame means of treatment will be found uſeful in both caſes. Of thoſe means it is not neceſſary to make particular mention in this place.

The bees themſelves are probably injured by the plants which afford noxious honey.

It would be curious to aſcertain, whether the bees are ever injured or deſtroyed by the quaffing of the nectar of the flowers from which they prepare the poiſonous honey. It is probable that they are; and, perhaps, ſome of the diſeaſes of theſe little inſects may ariſe from this ſource ‡. It is true, indeed, that there are ſome poiſonous plants the nectar of which the bees will not touch. This is the caſe with the fritillaria imperialis, or crown imperial §. I do not remember to have ſeen bees in, or immediately about, the flowers of the common roſebay, or oleander ||, in the tube of which there is a fluid which deſtroys thouſands of the common houſe flies. But what is called inſtinct is not always ſure. The bees may prepare an honey from plants that are very injurious to them. The excellent Mr. Evelyn, ſpeaking of the elm ſays, "but I hear an ill report of this tree for bees, that, furfeiting of the blooming ſeeds, they are obnoxious to the laſk **, at their firſt going abroad in ſpring, which endangers whole flocks, if remedies be not timely exhibited; therefore, 'tis ſaid, in great

* Hyoſcyamus niger.

† Datura ſtramonium.

‡ Dr. James E. Smith aſſerts that the honey or nector of plants is not poiſonous to bees. *Syllabus to a Courſe of Lectures on Botany*, p. 23. I have ſome good reaſon to believe that, ſometimes at leaſt, the contrary is the caſe.

§ Linnæus, ſpeaking of this plant, ſays, "Nulla, excepto *Melianthο*, copioſiori melle ſcatet planta, quam hæc; ſed apes id non colligunt!" Prælectiones in Ordines Naturales Plantarum. Edidit Giſeke. p. 297. Hamburgi, 1792.

|| Nerium oleander.

** This is one of the moſt mortal diſeaſes of bees. It is beautifully deſcribed, and the remedies for it mentioned, by Virgil, Georgic. lib. iv. l. 251---280.

elm countries they do not thrive; but the truth of which I am yet to learn *".

In South-Carolina, in Georgia, and in the two Floridas, but more especially in East-Florida, the instances of injuries from the eating of wild-honey are more numerous than in any other parts of North-America, that are known to us. Districts where this honey is often found.

There is a tract of country included between the rivers St. Illa and St. Mary's in East-Florida, that is remarkable for abounding in vast numbers of bees. These insects, which were originally introduced into Florida by the Spaniards †, have encreased into innumerable swarms, from the facility with which they procure their food, in perhaps the richest flowered-country of North-America. In this tract of country, the alarming effects of wild-honey are often experienced, by the settlers, by wandring hunters, and by savages.

It is highly probable, that this poisonous honey is procured from a considerable number of the flowers of the countries which I have mentioned. A complete list of these flowers would be acceptable: but such a list it will be difficult to procure at present. Perhaps, my hints may induce some intelligent native of the country to favour us with his observations on the subject. Meanwhile, I am happy to have it in my power to mention some of the vegetables from whose flowers the bees extract a deleterious honey, not only in the country between the St. Illa and St. Mary's, but also in some other parts of North-America. Desideratum. A list of the flowers that afford it.

These vegetables are the kalmia angustifolia and latifolia of Linnæus, the kalmia hirsuta of Walter ‡, the andromeda mariana, and some other species of this genus. Enumeration of some.

I. Every American has heard of the poisonous properties of the kalmia angustifolia and latifolia. The former of these plants is known, in the United States, by the names of dwarf-laurel, ivy, lambkill, &c. It has long been known, that its leaves, when eaten by sheep, prove fatal to them. The following fact will show that the flowers likewise are endued with a poisonous property. Kalmia angustifolia and latifolia.

* Silva: or a Discourse on Forest-trees, &c. p. 133 and 134. Dr. Hunter's edition.

† See Transactions of the American Philosophical Society. Vol. III. No. 31.

‡ Flora Caroliniana, p. 138.

About

Narrative of poisonous honey from those vegetables.

About twenty years since, a party of young men, solicited by the prospect of gain, moved, with a few hives of bees, from Pennsylvania into the Jerseys. They were induced to believe that the savannas of this latter country were very favourable to the encrease of their bees, and, consequently, to the making of honey. They, accordingly, placed their hives in the midst of these savannas, which were finely painted with the flowers of the kalmia angustifolia. The bees encreased prodigiously, and it was evident that the principal part of the honey which they made was obtained from the flowers of the plant which I have just mentioned. I cannot learn that there was any thing uncommon in the appearance of the honey: but all the adventurers, who eat of it, became intoxicated to a great degree. From this experiment, they were sensible that it would not be prudent to sell their honey; but, unwilling to lose all their labour, they made the honey into the drink well known by the name metheglin, supposing that the intoxicating quality which had resided in the honey would be lost in the metheglin. In this respect, however, they were mistaken. The drink also intoxicated them, after which they removed their hives.

In North-Carolina, this species of kalmia and the andromeda mariana are supposed to be the principal vegetables from which the bees prepare the poisonous honey that is common in that part of the United States.

The kalmia latifolia poisonous to animals,

II. That the kalmia latifolia, known in the United States by the names of laurel, great-laurel, wintergreen, spoon-haunch, spoon-wood, &c. is also a poison. Its leaves, indeed, are eaten, with impunity, by the deer *, and by the round-horned elk †. But they are poisonous to sheep, to horned-cattle and to horses. In the former of these animals, they produce convulsions, foaming at the mouth, and death. Many of General Bradock's horses were destroyed by eating the leaves and the twigs of this shrub, in the month of June 1755, a few days before this unfortunate General's defeat and death. In the severe winter of the years 1790 and 1791, there appeared to be such unequivocal reasons for believing that several persons, in Philadelphia, had died in

* Cervus Virginianus of Gmelin.

† Cervus Wapiti, *mihi*.

consequence

confequence of their eating our pheafant *, in whofe crops the leaves and buds of the kalmia latifolia were found, that the mayor of the city thought it prudent and his duty, to warn the people againft the ufe of this bird, by a public proclamation. I know that by many perfons, efpecially by fome lovers of pheafant-flefh, the circumftance juft mentioned, was fuppofed to be deftitute of foundation. But the foundation was a folid one. This might be fhown by feveral well-authenticated facts. It is fufficient for my prefent purpofe to obferve, that the collection of a deleterious honey from the flowers of this fpecies of kalmia gives fome countenance to the opinion, that the flefh of pheafants that had eaten of the leaves and buds of this plant may have been impregnated with a pernicious quality †.

and infecting their flefh.

I have been informed, that our Indians fometimes intentionally poifoned themfelves with a decoction of the leaves of this kalmia. The powder of the leaves has been employed (but I fufpect with little advantage) in the inflammatory ftage of certain fevers. From experiments made upon myfelf, I find that this powder is fternutatory.

Ufed for fuicide.

To fome conftitutions the flowers of the kalmia latifolia, even externally applied, are found to prove injurious.

III. The kalmia hirfuta appears to poffefs nearly the fame properties as the two fpecies which I have juft mentioned. This pretty little fhrub is a native of South-Carolina, Georgia, and Florida.

Kalmia hirfuta.

In Georgia and in Florida, this fpecies of kalmia is fuppofed to be the principal vegetable from which the deleterious honey in thofe parts of our continent is procured.

IV. The andromeda mariana, or broad leafed moorwort, is a very common plant in many parts of North America.

Andromeda mariana.

* Tetrao Cupido of Linnæus.

† It is not a new fufpicion, that the flefh of animals that have eaten of the leaves, &c. of deleterious vegetables is fometimes endued with a poifonous property. Georg. H. Welfchius, a very learned German writer, quoted by Dr. Haller, (See *Hiftoria Stirpium Indigenarum Helvetiæ Incheata*. Tom. I. p. 433.) fays, that the flefh of a hare which was fed with the leaves of the rhododendron ferrugineum proved mortal to the guefts: This fpecies of rhododendron is a native of Switzerland, Siberia, and other parts of the old world.

The

The leaves are poiſonous to ſheep. The petioli, or footſtalks of the leaves and the ſeeds, within the ſeed-veſſel, are covered with a brown powder, ſimilar to that of kalmiæ. This powder applied to the noſtrils occaſions violent ſneezing *. From the flowers of this plant, the bees extract conſiderable quantities of honey; and it deſerves to be mentioned that this honey, as well as that obtained from ſome other American ſpecies of andromeda, has frequently the very ſmell of the flowers from which it is obtained †.

Plants affording poiſonous honey.

I have already obſerved, that it is highly probable, that the American poiſonous honey is procured from the flowers of a conſiderable number of the plants of the country. I have mentioned but a few of them. But there are many others which I have ſome reaſons for ſuſpecting are alſo capable of affording an injurious honey. Indeed, every flower that is poiſonous to man, and is capable of affording honey, may produce an honey injurious to man; ſince the properties of this fluid are ſo dependant upon the properties of the plants from which it is procured. There is, therefore, more poetry than philoſophy in the following lines of Mr. Pope;

"In the nice bee, what ſenſe ſo ſubtly true,
"From pois'nous herbs extracts the healing dew."
ESSAY ON MAN, Epiſtle I. lines 211 & 212.

I have been informed that, in the ſouthern parts of our continent, there is a plant, called hemlock, from the flowers of

* For ſome information relative to the properties of the andromeda mariana, ſee Collections for an Eſſay towards a Materia Medica of the United States, pages 19, 20, 47. Philadelphia, 1798.

† In juſtice to the fine genus of andromeda, I muſt obſerve, that all the ſpecies do not furniſh a pernicious honey. The andromeda nitida or lucida of Bartram affords an abundance of nectar, or honey. The flowers of this ſpecies are called by the country people of Carolina and Georgia, "honey flowers," not, however, merely from the circumſtance juſt mentioned, but from the regular poſition of the flowers on the peduncle, which open like the cells of a honey-comb, and from the odour of theſe flowers, which greatly reſembles that of honey. This ſpecies grows abundantly in the ſwamps called bay-galls. The inhabitants of Carolina are univerſally of opinion, that it affords the greateſt quantity of honey, and that of the beſt quality.

which

which the bees prepare a honey that is poiſonous. The flowers are ſaid to be yellow, and the root a deadly poiſon. I do not know what plant this is. Moſt probably, it is ſome umbelliferous plant, perhaps a cicuta, an angelica, or a ſcandix. Plants affording poiſonous honey.

Some ſpecies of agaricus, at leaſt ſome fungous vegetables, that grow in the ſouthern ſtates, are extremely poiſonous. As accidents from the uſe of deleterious honey have happened in the ſame countries in which theſe poiſonous fungi grow, it has been ſuppoſed, and aſſerted, that the poiſonous honey is prepared from a dew that collects upon theſe fungi. Perhaps, this ſuppoſition is not entirely devoid of foundation *.

I ſhall now mention a few vegetables from the flowers of which, I think, it will be found, that the bees collect a poiſonous, or injurious honey. Theſe are:

* If the celebrated author of the *Recherches Philoſophiques ſur les Americans* be ſtill living, this account of our poiſonous and injurious honey (ſhould my memoir fall into his hands) would afford him ſome entertainment. I would adviſe him to connect the facts, which I here communicate, with the remarks concerning our inſects contained in the firſt volume of the *Recherches* (ſee p. 169 and 170.) I hope, however, that Mr. De Pauw, who, notwithſtanding his love of ſyſtem and his many errors, is certainly a man of great reading, will recollect, that the Greek and Roman writers (as we ſhall afterwards ſee) have ſaid much concerning the poiſonous honey of various parts of the old world. And now let me add, that in America there is as good honey as in any other parts of the world; and there is not a ſcarcity of this good honey. The honey which is collected from the flowers of the tulip-tree (liriodendron tulipifera), the buckwheat (poligonum fagopyrum), the red-maple (acer rubrum), the clover (trifolium), and many other plants is excellent. The Abbe Clavigero ſays, the bee of Yucatan and Chiapa makes "the fine clear honey of Eſtabentun, of an aromatic flavour, ſuperior to that of all the other kinds of honey with which we are acquainted." (A) *The Hiſtory of Mexico*, Vol. I. p. 68. Perhaps on ſome future occaſion, I may communicate to the Philoſophical Society a liſt of thoſe indigenous vegetables which, as furniſhing an innocent and excellent honey, are worthy of preſervation in the neighbourhood of apiaries. The liſt is an extenſive one.

(A) This fine honey, according to the Mexican hiſtorian, is "made from a fragrant white flower like jeſſamine, which blows in September."

I. The

Plants affording poisonous honey.

I. The rhododendron maximum, or Pennſylvania mountain laurel. This belongs to a very active genius of plants. We have already ſeen that one of the ſpecies, the rhododendron ferrugineum, was, long ago, obſerved to produce the ſame effects which have been aſcribed to the kalmia latifolia. Another ſpecies, the rhododendron cryſanthum, has been found a powerful medicine, and has been uſed in Ruſſia, with much advantage, in the iſchias, in chronick-rheumatiſm, and in other diſeaſes; and we ſhall immediately ſee that from another ſpecies a poiſonous honey has been procured in the neighbourhood of the Euxine-Sea. The footſtalks of the leaves, and alſo the ſeeds, of our rhododendron maximum, are covered with the ſame brown powder as I obſerved covered the leaf-footſtalks and the ſeeds of ſeveral of the andromedæ, and the kalmiæ. This powder in the rhododendron, as well as in the andromedæ and kalmiæ, excites ſneezing, and it is curious to obſerve that a ſneezing is mentioned by Dioſcorides among the ſymptoms produced by the honey about Heraclea Pontica. That honey, as will be preſently ſhown, is procured from the rhododendron ponticum.

II. The azalea nudiflora. This fine ſhrub is well known in Pennſylvania, and other parts of the United States, by the name of wild honeyſuckle. Of its properties I know nothing certain. It has, however, too much of the family face, and is too frequently found in company with the rhododendron maximum, and the kalmiæ, not to make me ſuſpicious that it partakes alſo of the characters of theſe deleterious vegetables. Moreover, a ſpecies of this genus, the azalea pontica of Linnæus, is ſuppoſed to be the ægolethron of Pliny, who mentions it as the plant from which the poiſonous honey about Heraclea Pontica is prepared. The tube of the flower of our azalea is perforated by the large bee, called bumble-bee.

III. Datura ſtramonium. This plant is known by a variety of names, ſuch as Jameſtown-weed, gymſin; ſtink-weed, French-cheſnut. Its active and poiſonous properties are now pretty generally known. Children have often been injured by eating the ſeeds. The tube of the flower contains a conſiderable quantity of honey. This honey is bitter, and has much of the poiſonous ſmell. Bees quaff it. But admitting that it is of a poiſonous nature, it does not follow that our *cultivated* bees (if I may be allowed to uſe this expreſſion) will

will collect ſo much of this honey as to prove injurious to thoſe who eat of it. But, in particular places, where this plant has been permitted to increaſe to a great degree, large quantities of honey may be collected from it: and I cannot help ſuſpecting that the uſe of this honey may prove injurious *.

Ancient accounts of noxious honey.

Some of the ancient writers of Greece and Rome have related inſtances of the deleterious properties of the honey of certain countries. The botaniſt Dioſcorides, ſpeaking of the rhododendron ponticum, a ſpecies of the ſame genus to which our mountain laurel belongs, has the following words: "About Heraclea Pontica, at certain ſeaſons of the year, the honey occaſions madneſs in thoſe who eat of it; and this is undoubtedly owing to the quality of the flowers from which the honey is diſtilled. This honey occaſions an abundant ſweating, but the patients are eaſed by giving them rue, ſalt-meats, and metheglin, in proportion as they vomit. This honey," continues the Greek botaniſt, "is very acid, and cauſes ſneezing. It takes away redneſs from the face, when pounded with coſtus. Mixed with ſalt or aloes, it diſperſes the black ſpots which remain after bruiſes. If dogs or ſwine ſwallow the excrements of perſons who have eaten of this honey, they fall into the ſame accidents †.

Pliny has alſo taken notice of this poiſonous honey. "In ſome years," ſays the Roman naturaliſt, "the honey is very dangerous about Heraclea Pontica. It is known to authors from what flowers the bees extract this honey. Here is what we have learned of the matter. In thoſe parts, there is a plant called ægolethron, whoſe flowers, in a wet ſpring, acquire a very dangerous quality, when they fade. The honey which the bees make of them is more liquid than uſual, more heavy, and redder. Its ſmell cauſes ſneezing. Thoſe who have eaten of it ſweat exceſſively, lie upon the ground, and call for nothing but cool drinks ‡". He then makes the very remarks which I have quoted from Dioſcorides, whoſe words, indeed, as Mr. Tournefort obſerves, he ſeems to have merely tranſlated. The following remark, however, appears to be-

* See the late Dr. Samuel Cooper's inaugural diſſertation on the properties and effects of the Datura Stramonium. p. 33. Philadelphia, 1797.

† Dioſcorides, as quoted by Mr. Tournefort.

‡ C. Plinii Secundi Naturalis Hiſtoriæ, Lib. XXI. cap. xiii.

long

Ancient accounts of noxious honey.

long to Pliny: "Upon the same coast of the Pontus, there is found another sort of honey, which is called mœnomenon *, because those who eat of it are rendered mad. It is supposed, that bees collect it from the flowers of the rhododendros, which is common among the forests. The people of those parts, although they pay the Romans a part of their tribute in wax, are very cautious how they offer them their honey †."

The Greeks and the Romans have often described the various plants that were known to them, in such dark and obscure terms, that the botanists of modern times are frequently at a loss to determine, not merely the species but also the genus the ancient writers have mentioned. With respect, however, to the plants which I have just mentioned, the difficulty does not seem to be great. Mr. Tournefort has, I think, shown, in a very satisfactory manner, that the ægolethron of Pliny in the chamæ-rhododendros pontica maxima, Mespili folio, flore luteo of his *Institutiones*, a plant since described by Linnæus, and by other botanists, by the name of azalea pontica. Mr. Tournefort has likewise shown, that the other plant called by Pliny rhododendros in his chamærhododendros pontica maxima, folio laurocerasi, flore cœruleo purpurescente ‡. This is the rhododendron ponticum of Linnæus. It is considerably allied to the azalea pontica.

Xenophon has recorded the remarkable effects of some poisonous honey, in his celebrated work, called *Memorabilia*.

When the army of the ten thousand had arrived near Trebisond, on the coast of the Euxine or Black Sea, an accident befel the troops which was the cause of great consternation. "As there were a great many bee-hives," say the illustrious general and historian, "the soldiers did not spare the honey. They were taken with a vomiting and purging, attended with a delirium, so that the least affected seemed like men drunk, and others like mad men, or people on the point of death. The earth was strewed with bodies, as after a battle; not a person, however, died, and the disorder ceased the next day, about the same hour that it began. On the third and fourth days,

* From the Greek verb, Μαίνομαι, insanio.

† Ibid.

‡ Institutiones, &c.

days, the ſoldiers roſe, but in a condition people are in after taking a ſtrong potion *." Ancient accounts of noxious honey.

The ſame fact is recorded by Diodorus Siculus.

Mr. Tournefort thinks there is every probability that this poiſonous honey was ſucked from the flowers of ſome ſpecies of chamærhododendros, or rhododendron. He obſerves that all the country about Trebiſond is full of the ſpecies of this plant, and he remarks that Father Lambert, Theatin miſſionary, agrees thut the honey which the bees extract from a certain ſhrub in Colchis or Mingrelia, is dangerous, and cauſes vomiting. Lambert calls this ſhrub oleandro giallo, or the yellow roſe-laurel, which Mr. Tournefort ſays is, without diſpute, his chamærhododendron pontica maxima, Meſpili folio, flore luteo †; the azalea pontica, already mentioned.

There are ſeveral paſſages in the Roman poets, which plainly ſhow, that they were no ſtrangers to the poiſonous properties of certain kinds of honey. It is not neceſſary to mention all theſe paſſages. But the following are worthy of notice.

Virgil cautions us not to ſuffer a yew tree to grow about bee-hives:

Neu propius tectis taxum ſine.——————

GEORGIC. Lib. IV. l. 47.

In his 9th Ecologue, the ſame philoſophic poet ſpeaks of the yews of Corſica as being particularly injurious to bees.

Sic tua Cyrnæas fugiant examina taxos. l. 30.

The honey of Corſica, as Dr. Martyn ſtrongly expreſſes it, " infamous for its evil qualities ‡."

The

* Theſe are nearly the words of Mr. Tournefort's tranſlation. I am ſorry that I have not the original work of Xenophon at hand.

† See Tournefort's Voyage into the Levant. Vol. iii. p. 68. Engliſh tranſlation. London, 1741.

‡ See his Tranſlation of the Georgics of Virgil, note to line 47, in book IV. Dr. Martyn's criticiſms and annotations always demand attention. I greatly doubt, however, if the taxus of Virgil be the common yew, or any ſpecies of that genus. Martyn himſelf allows, that " it does not appear from other writers (beſide Virgil), that Corſica abounded in yews." I have been aſſured, that the yew is not an indigenous vegetable in that iſland, and that it is even rare among the foreign vegetables. It may, indeed, be ſaid, perhaps it was common in the time of Virgil. I would obſerve, that the yew is much leſs poiſonous than has been commonly ſuppoſed

Advantages of a proper attention to the plants near bee hives, &c.

The raifing of bees, for the purpofes of procuring their honey and their wax, may, at fome future period, become an object of great importance to the United States. Surely then, it would be a matter of confequence to attend to the cultivation or prefervation of thofe vegetables which furnifh an innocent and a well-flavoured honey, and a good wax. But even in a more limited view of the fubject, fome knowledge of thefe vegetables feems to be indifpenfibly neceffary. And in the new fettlement, whither the fettler has carried his bees, where improvements are ftill very imperfect, it cannot be deemed a trivial tafk to have pointed out fome of thofe vegetables from which an injurious honey is obtained.

The ancients, who, in fome refpects at leaft, were equal to the moderns, appear to have paid much attention to this fubject. Virgil * and Columella have both told us what plants ought to grow about apiaries. It is unneceffary to repeat, in this place, what the two Roman writers have faid on the fubject. The *Georgics* of the Mantuan poet are in the hands of every man of tafte; and the work of Columella † *fhould* be read, wherever agriculture engages the attention of gentlemen.

The proper management of bees may be confidered as a fcience. It is not fufficient that bees merely make honey and wax. Their honey may be injurious or poifonous, and their wax may be nearly ufelefs. To affift, and to direct the labours of thefe little infects, the knowledge and the hand of man are

pofed. I know not that any modern writer has pretended that the bees procure a pernicious honey from its flowers. Thefe facts give rife to my fufpicion, that the taxus of Virgil was not the yew, or taxus of the modern botanifts. If not the yew, what vegetable was it? Perhaps, the buxus virens, or box. This vegetable abounds in Corfica, where to this day it is known by the name of *taxo*. The gentleman from whom I received this information affured me, that the bees of Corfica are very fond of the flowers of the box, and that the honey from this fource is reputed poifonous. The box is, unqueftionably, a poifonous vegetable. But there is ftill a difficulty in the cafe. Virgil mentions both taxus and buxus. I think there can be no doubt that his buxus (fee Georgic. lib. II. l. 449.) is the buxus of the modern botanifts.

* See Georgicorum, lib. IV. l. 30.—32.

† De Re Ruftica, libri XII.

required.

required. Let, then, this interefted being be at leaft attentive to his own benefits and pleafures. Let him carefully remove from about the habitations of his bees every fetid or poifonous vegetable, however comely its colour or its form. In particular, let him be careful to remove thofe vegetables which are noxious to himfelf. In place of thefe, let him fpread the "marjoram and thyme," and other plants, "the love of bees*," and his labours will be rewarded. He may, then, furnifh his table with an honey not inferior to that of Mount Hermettus, or of Athens; nor to that of Sicily, to which Virgil has fo handfomely alluded in the feventh Eclogue:

> *Nerine Galatea, thymo mihi dulcior Hyblæ,*
> *Candidior cygnis, hederâ formofior albâ.*
>
> L. 37, 38.

V.

On the Compofition and Ufe of Chocolate. By CITIZEN PARMENTIER †.

AMONG the fubftances with which the conqueft of the new continent has enriched the old, muft be placed the cocoa or cacao nut. From this fruit, or rather from this feed, it is that the Mexicans have, from time immemorial, prepared their favourite beverage, chocolate: This confifts of cocoa roafted and bruifed, which they mixed by ftirring water, and added the flour of maize to give it confiftency, together with pimento to flavour it. The exiftence of fugar was unknown to them, becaufe the cane, which is indigenous to India beyond the Ganges, was not brought to Saint Domingo by Defticaca until 1506; and becaufe Balaftro was the firft who fubmitted this plant to the operation of the mill, in America ‡.

Preparation of chocolate by the Mexicans.

Importation of the fugar cane into the Weft Indies and America.

The Spaniards partook of the enthufiafm of the Mexicans in the wonderful properties which they afcribed to chocolate; and its preparation, notwithftanding the little fkill it requires, foon became in their hands an object of fpeculation; they kept

The preparation of chocolate kept fecret by the Spaniards.

* Armftrong.

† From the Annales de Chimie, No. 134, tom. XLV, 139.

‡ See an excellent fketch of the only hiftory of Sugar, by Dr. Falconer, in our Journal, quarto feries, II. 136.

it ſecret, and ſold, and continue to ſell to other nations, a ſimple paſte of cocoa roaſted, bruiſed and brought into the form of cylindrical rolls, for chocolate.

Sugar added to it.

The uſe of ſugar having been rendered more common in Europe by the introduction of the cane into our colonies, it ſoon became the univerſal condiment, and the Spaniards did not fail to make it an addition to the preparation of chocolate, for the purpoſe of correcting its unpleaſantneſs to thoſe who were unaccuſtomed to this beverage; but it was not till ſome time afterwards that other nations made the diſcovery that cocoa was its baſe, ſugar its ſeaſoning, and cinnamon and vanilla its aromatic ingredients. This diſcovery became a ſource of wealth to a number of individuals, who in their turn made a myſtery of it; hence aroſe the chocolates of Italy, of Portugal, and of Spain, which, compared with thoſe prepared in Paris, and the other towns of France, poſſeſs no ſuperiority. Why ſhould we allow thoſe countries to have ſuch an advantage over us? The ingredients which form chocolate are not cultivated in them any more than with us, we all obtain them from the ſame ſources and at the ſame charges. At Naples, Liſbon, and Madrid, the chocolate of France is in great eſteem; but it is an eſtabliſhed maxim in all countries that one ſhall be thought a prophet at home, and this proverb may alſo be applied to chocolate.

Diſcovered by other nations.

Extraordinary virtues attributed to it.

I ſhall not enquire whether chocolate really deſerves the encomiums beſtowed on it, or whether it be advantageous in all the caſes in which its uſe is recommended. If we credit the writings of the phyſicians of the two worlds, nature has formed the cocoa as a remedy for all the evils that afflict the human race, and as the means of prolonging life beyond its accuſtomed limits. But it is difficult to guard againſt exaggeration, particularly on the ſubject of productions which frequently have no other value but that ariſing from their growing at a diſtance from us, and under another hemiſphere. From experience and obſervation we have learned nothing more than that chocolate is an agreeable aliment, light and of eaſy digeſtion; it is for that reaſon given particularly to convaleſcents, to perſons of delicate habits, and to the aged. In fact, to obtain its good effects uniformly, it is neceſſary that the ingredients of which it is compoſed ſhould be properly ſelected, well prepared, and intimately mixed, ſo as to form a ſoft, homogeneous paſte.

Whether well founded.

On

On the other hand, on confidering the nature of the fubftances which form chocolate, and the mode in which their combination is produced, we are forced to acknowledge all the characteriftics of a preparation truly pharmaceutical, and to allow that it requires a degree of care and attention, of which ordinary labourers are incapable without fuperintendance and direction.

It is a pharmaceutical preparation,

The felection of cocoa is not fufficient to give that quality to chocolate which it ought to have: this fruit muft be fifted, then roafted by a gentle heat to deprive it of its humidity and to develope the odour and flavour which belongs to it; it muft be peeled grain by grain to feparate the bark, the germ or radicle and thofe nuts which appear to be fpoiled; the cocoa, thus picked, muft be ground for a long time with a certain quantity of the fugar on a ftone, gradually heated on a fand-bath; the remainder of the fugar is not to be added until the fecond bruifing, and the aromatic ingredients, which are pounded with the fugar, are not to be put in till near the clofe of the operation. It is then divided into a mafs of a fuitable weight, and fafhioned in tin moulds, whence it is taken when cold to be wrapped up, care being taken to keep it in a dry cold place: the winter is the moft favourable feafon for this operation.

and requires to be carefully made up.

Since the various fpecies of cocoa which are found in commerce could not, were each treated feparately, produce chocolate of a good quality; it is ufual to mix them in proportions which are determined by the price intended to be charged, and by the tafte or fancy of the confumer; when it is flavoured by cinnamon alone, it is called *chocolat de fanté*: it bears the name of chocolate of half, one, two, or three vanilloes, when there are a half, one, two, or three pods of that fruit in a pound of it, becaufe the weight of the pods are unequal.

Preparation of chocolate for commercial purpofes.

Chocolate thus compofed is infinitely preferable to the crude pafte of cocoa which is ftill prepared in the Antilles, and which the Spaniards continue to fend to us, under the pretence that it is more commodious in that ftate, becaufe the fugar and aromatic ingredient may be added in the requifite proportion when ufed, but the chocolate thus made does not poffefs a perfect homogenity. The butter of cocoa conftantly rifes to the furface; for when the fugar is bruifed with cocoa in the ftone, during the mixture, a more intimate combination of all the principles

Superior to that brought from the Antilles.

principles is effected. In a word, the chocolate thus obtained is more completely diffolved, more mifcible in water and confequently more eafy of digeftion: on the other hand, although cocoa does not become rancid fo eafily as the fruits which are analogous to it, it may be apprehended that from being expofed too fuddenly to the action of the heat neceffary to bruife the nut, the oil and the butter which it contains being fet free, it may lofe part of its mildnefs and become acrid and heating.

Of the Frauds committed in the Fabrication of Chocolate.

Frauds in the manufacture.

It is, no doubt, unfortunate, that in the commerce of food, in which the love of humanity, that fentiment fo pure and fo natural, might feem to banifh all want of confidence, and every fordid intereft, we neverthelefs difcover that frauds are multiplied in proportion as the objects pafs into different hands to acquire their alimentary or medicinal properties. It is not my intention to attach perfonal blame to any individual, but the improper conduct of fome manufacturers render it neceffary to give precautions, which are, no doubt, ufelefs with regard to others, who fulfil the duties of their ufeful profeffion with a degree of fidelity worthy of the beft of times.

Partly noticed by others.

Baumé, in his Elements of Pharmacy, and *Demachy*, in his Art of the Liqueur-maker, have difcovered part of the abufes which are committed in the fale of chocolate. I fhall feel happy if, by adding fome obfervations to what has been already publifhed by thofe chemifts, I fhould fucceed in preferving the juft reputation it deferves, and which it has loft in the opinion of fome individuals only by the faults of the preparation, or by the addition of materials foreign to its compofition.

Inftance of the bad effects arifing from foreign ingredients.

Among the number of perfons I have heard complain againft chocolate, I fhall inftance a lady of a tolerably good conftitution, to whom it had been prefcribed as a medicine; the ill effects fhe experienced from it gave me reafon to fufpect the purity of her chocolate: I examined it, and found that it contained a quantity of farinaceous matter. Now this fubftance had been exprefsly forbidden by her phyfician; I perfuaded her not to difcontinue the ufe of chocolate, but to procure it elfewhere: the uneafinefs, the oppreffion, and the acidity which had tormented her, foon difappeared, and her ftomach was infenfibly reftored. Thus the means to which fhe ought to have been indebted for her cure, were likely to have proved the caufe of her deftruction.

I thought

I thought it a duty to take this opportunity to examine other chocolates bought indifcriminately from feveral makers; fome I found to be faithfully prepared; but others contained the farina of wheat, others the farina of lentils, of peas and of beans, and laftly, the fecula of potatoes. It will be faid that thefe fubftances are not injurious to the animal economy; and to that I agree. But in circumftances in which chocolate makes part of a regimen, and is prefcribed as a medicinal food, they cannot but be prejudicial to the health: befides, why introduce them into it? they are intirely foreign to the compofition of chocolate. Thefe obfervations apply to all the additions which are mentioned in advertifements, and fuppofed by fome perfons to be improvements. Examination of various kinds. The added ingredients though not unwholefome may be improper in the medicinal ufe.

But admitting what is not true, that it is neceffary to make chocolate thicker and more fubftantial, the mixtures here mentioned ought not to be made till the moment of its preparation, and, if I may be allowed the expreffion, under the eye of the confumer. I muft obferve that, if it be thought ufeful to add farinaceous matters, they fhould always be employed in the ftate of fecula or ftarch, becaufe in that ftate they are deprived of glutinous and extractive matter, and contain the alimentary principle only. Any addition improper,

The compofition of chocolate ought to be diftinguifhed from its preparation, or making up; the latter is in the province of the confumer, who may add at his pleafure the yolks of eggs, for the purpofe of giving to his beverage a more faponaceous character, and make ufe of milk inftead of water to increafe its nutritive properties. It has even been remarked that many who are unable to take milk without the immediate production of acidity, have fucceeded in digefting it by the help of a little chocolate; but I again repeat, that if it fhould be the defign of a phyfician to prefcribe it, it is a medicine on whofe effects he cannot rely, while its compofition continues arbitrary and uncertain. unlefs made by the confumer.

There are other frauds ftill more injurious to the effects of chocolate, which I have alfo difcovered in my examination; fome makers procure, at a low price, the refufe of cocoa pafte, from which the butter has been extracted, and replace it by oils or animal fats; others add roafted almonds, gum tragacanth, or gum arabic; and laftly there are fome who provide themfelves with acrid, bitter, and newly gathered cocoa-nuts, be- Injurious frauds.

 caufe

caufe thefe articles, which are always to be had at low prices, are capable of bearing a greater quantity of fugar, which proportionally diminifhes the prime coft of the chocolate.

Bad qualities produced by want of fkill,

We muft alfo obferve that, with the beft intentions, the chocolate may be of inferior quality, without containing any foreign ingredient, becaufe the fubftances in its compofition may have been ill-felected, or the firft operation may have been, in fome refpects, carelefsly prepared: the whole art confifts in choofing the quality of the cocoa, and above all, in avoiding either extreme in roafting it; if it be too flightly roafted, it retains a difagreeable tafte; if it be burned, it not only acquires bitternefs, but the liquid prepared from it is black and wants that unctuofity which is fo much admired in it; and laftly, if the germ be not feparated from the two lobes of the fruit, its hard and horny ftate, refifting the action of the bruifing and the boiling, it will be found entire at the bottom of the cup of chocolate: its prefence is fufficient to fhow that the firft work, which confifts in peeling the cocoa grain by grain, has been neglected, and that no more care has been beftowed on the fubfequent operations.

or by careleffnefs in the fabrication.

Another obfervation is that the greater number of the workmen to whom the fabrication of chocolate is confined require much overlooking on the part of the mafter; they may be unfaithful when they are employed by tafk-work; they will bruife and work the pafte carelefsly, and, to fave labour and time, they give a degree of heat which is too powerful and particularly injurious to the quality of the chocolate.

Methods of difcovering the Frauds.

Methods of difcovering frauds.

It is not enough to have pointed out the frauds practifed in the fabrication of chocolate, and all the defects of negligence or choice in the qualities of the materials and in the preparation. We fhall have only accomplifhed half our object, if we do not enable the confumer to diftinguifh them fo as not to be miftaken.

Qualities of chocolate when genuine.

Thofe who are accuftomed to pure chocolate can eafily judge of its goodnefs: its fracture ought to fhow nothing of a granulated appearance. On tafting it, it ought to melt in the mouth, and leave a fort of frefhnefs; and laftly, in making drink of it, it ought to acquire, either with water or with milk, only a moderate confiftence.

 In

In all cafes when the chocolate leaves a pafty tafte in the mouth; when in preparing it, the liquor exhales on the firft boiling a fmell of glue; and when after its entire cooling it is converted into a fpecies of jelly, we may be certain that the chocolate contains farinaceous matter, in quantity proportioned to the degree of the effects here pointed out: if it depofits at the bottom of the cup, fmall hard bodies, and an earthy or gravelly fediment, it is a proof that it has not been well picked and that raw fugar, more or lefs coarfe, has been ufed inftead of refined fugar. The fmell of cheefe difcovers the prefence of animal fats, and rancidity, or that of emulfive feeds; and the bitter, faline or mufty flavour announces that the cocoa employed was too green, too much roafted or decayed.

Probity and care neceffary in the preparation.

It cannot be too often repeated that chocolate is not an indifferent preparation; it does not require fcience, but probity and care; the makers of chocolate ought to leave the confumers the right to add what they pleafe, when they are defirous either to increafe its efficacy or its pleafantnefs, according to their own choice. They are often led by improper confidence, or an economy ill underftood, to take it of an inferior quality; for chocolate in fact poffeffes a real value, notwithftanding which, many are unwilling to pay more than half its fair price, while others pay much too dear for it.

The limits of a notice will not permit us to point out the characters which diftinguifh the cocoas of commerce from each other, nor to afcertain here at what price the chocolate can be obtained by thofe who fabricate it: thefe details fhall be the fubject of a feparate memoir.

General refult.

It refults from what I have faid, that chocolate is not at prefent what it was when the Spaniards conquered Mexico, at the beginning of the fixteenth century; that no peculiar method is required for its preparation; that though the proportions of the elements which compofe it may be varied, the procefs for applying and difpofing them to form a good compound, muft be conftant and invariable; that its quality depends upon the choice of the ingredients and the care employed in combining them; that negligence, avarice, and quackery change its nature fo much as to convert it into a heavy, indigeftible and heating fluid; that in order to procure it with all the qualities which characterife good chocolate, it is neceffary to buy of dealers of good character, and at a fair price; and laftly, that every

every manufacturer who admits into the compofition of chocolate, materials which ought not to form a part of it, or at leaft, which the confumer does not require, directly injures his health. He who adulterates an article of general ufe, ought to be aware that though he conceals himfelf in the obfcurity of his work-fhops to introduce low priced ingredients into his chocolate, and to difguife them, yet he cannot efcape from a chemical analyfis, which can inftantly difcover his frauds, and denounces his pernicious art and his name to the public notice.

Additional obfervations by Cadet.

To the obfervations contained in this notice, I fhall add thofe which citizen *L. C. Cadet* has communicated to me on the chemical analyfis of certain foods. I fhall ufe his own words;

" The prefect of the police lately charged me to analyfe fome chocolate which he fufpected to contain fome noxious fubftances. I fhall not enter into the detail of the multiplied experiments which this examination required, but fhall confine this notice to a fingle remark, founded on the following fact:

Un-manufactured chocolate was found to contain a metallic precipitate

" To difcover whether this chocolate contained any metallic fubftance, I incinerated it and wafhed the afhes with very pure nitric acid, which retained every thing that was foluble.

The filtered lixivium was clear, but I had fcarcely poured into it a hidro-fulphuret, when I obtained a very abundant, black, metallic precipitate. This refult gave me much uneafinefs, becaufe I was ftill ignorant of the nature of the metal, and it is always diftreffing to find a pernicious fubftance in an article of food. I tried the lixivium with pruffiate of potafh. Immediately a beautiful Pruffian blue was formed, which removed my apprehenfions. I continued the examination with different re-agents, and the oxalic acid fhowed me the prefence of lime. It was interefting to learn whether the iron and the lime which I had difcovered would be met with in all chocolates: I then made an analyfis of the Caracca cocoa and of fome of the beft from the plantations: I did not find in them either lime or metal. I immediately caufed fome chocolate to be prepared from the fame cocoa and very white cryftallized fugar; the analyfis difcovered a pretty large proportion of iron and lime. It is, therefore, the fabrication which introduces thefe two fubftances into the chocolate, and the quantity is greater in proportion to the care beftowed on the manipulation. In fact the cocoa is roafted in a cylinder of fheet iron fimilar to thofe ufed in roafting coffee. It is afterwards beat in an iron mortar and

— of iron,

and alfo lime,

which are not found in the nut but are introduced by the prefent methods of working.

and finally ground with an iron roller on a calcareous ſtone, the ſurface of which is worn by the friction, and affords the lime.

Propoſed alterations.

" If the cocoa was roaſted in a veſſel of leſs oxidable metal, or in well baked earthen-ware; if the paſte was ground on a granite or a porphyry with a roller of the ſame materials, the chocolate would contain neither lime nor iron. In manipulations on a large ſcale, metallic ſubſtances are often introduced into the ſubjects of operation. Hence the extract of tamarinds, and more particularly the extract of liquorice (Spaniſh juice,) contains ſo great a quantity of copper, that this metal is frequently viſible at the firſt inſpection; for the ſame reaſon apothecaries are obliged to purify them to avoid thoſe ſerious accidents they might occaſion.

Proportions of iron and lime introduced.

" I was curious to know what was the proportion of metal and earth thus introduced into the chocolate. I repeated the experiments with care on quantities accurately weighed, and I aſcertained that five hectogrammes (one pound) contained twenty-four decigrammes (forty-eight grains) of lime, and twenty decigrammes (thirty-ſix grains) of iron; this proportion is the minimum. Thus a man who takes a cup of chocolate daily, at the end of the year has eaten eight hundred and ſixty-four decigrammes (three ounces) of lime, and ſeven hundred and forty decigrammes (two ounces two gros) of iron.

Utility of analyſing aliments.

" As iron is a ſalubrious metal and as the proportion of the lime in the chocolate is not very conſiderable, no uneaſineſs need be formed reſpecting the uſe of this aliment; nevertheleſs, I am of opinion that phyſicians will perceive the utility of collecting the analyſes of the different ſubſtances uſed for the nouriſhment of mankind. Until our perfect growth, the developement and ſolidification of our bones requires that we ſhould abſorb from our food a certain quantity of lime; and we recollect with intereſt the labours of Citizens Vauquelin and Alexander Brogniart, who found lime in flour, and who calculated that a man who conſumes only one pound of bread in a day will have eaten near two pounds of lime at the year's end.

Phyſiological remarks.

" When a man has arrived at the ſummit of his growth, he has no longer occaſion for the ſame quantity of lime; hence it is found abundantly in his urine and excrements; but as this earth, in the different digeſtive paſſages meets with different

 acids

acids which combine with it and form ſalts, the greater number of which, ſuch as the phoſphate and oxalate are but little ſoluble, it frequently produces diſorders in the moſt neceſſary organs of life; it is known that it is the baſe of moſt of the urinary calculi and arthritic concretion. It is therefore highly important to the progreſs of medicine, to examine the aliments which contain this earth, in an oſtenſible manner. The Citizens Vauquelin and Alexander Brogniart, found it in abundance in bread: I have diſcovered it in chocolate.

" Citizen Delaville, in a memoir which made part of the hundred and twenty-ſecond number of the *Annales de Chemie*, ſays, ſpeaking of the ſap of cabbages, " this ſap evaporated yields a conſiderable quantity of ſulphate of lime, the ſap of radiſhes affords reſults nearly ſimilar."

" Here then are four aliments which are frequently employed at the ſame time, and which carry into the paſſages of digeſtion a conſiderable quantity of lime. Other inſtances might be cited, ſuch as ciders ameliorated by the merchants with chalk, and I have thought theſe facts were not unworthy of the attention of phyſicians. It is the aſſemblage of ſuch obſervations, trifling in appearance, which frequently explain the cauſe of the phenomena belonging to phyſiology. Nothing ſhould be neglected in animal chemiſtry, the principal aim of which is the preſervation of our health and the annihilation of the moſt dreadful maladies which afflict human nature.

VI.

On the Cauſes by which the Oxigen of the Atmoſphere is ſupplied or renovated. In a Letter from Mr. ROBERT HARRUP.

To Mr. NICHOLSON.

SIR,

IF you think the incloſed paper of ſufficient importance to meet the public eye, be ſo kind as to give it a place in your valuable and intereſting Journal.

I am, Sir,

your obedient humble ſervant,

ROBERT HARRUP.

Chobham, June 22, 1803.

WHEN

WHEN we confider the great importance of oxigen in the fyftem of nature, its almoft univerfal agency and the immenfe quantities which are every inftant confumed on the furface of the globe, the queftion naturally arifes, from what copious fources are we fupplied with this wonderful fubftance in a gafeous ftate? As neither animal life nor combuftion can be maintained one moment without it, the quantity daily deftroyed muft exceed all calculation. To form fome diftant idea of this confumption, let us endeavour to afcertain the leaft poffible quantity which muft neceffarily be decompofed in a given time, in a large populous city. It is proved by experiment, that one perfon confumes about five cubic feet of atmofpheric air in an hour, or, in other words, decompofes the oxigen gas contained in that quantity. Now, if this gas is taken at one fourth of the whole, it will be found that 100 perfons decompofe 125 cubic feet every hour, and if the population of London be taken at 800,000 perfons only, the quantity decompofed every hour, will amount to no lefs than one million cubic feet. But if this quantity is deftroyed by refpiration alone, how much more muft be confumed by combuftion in the fame fpace of time. It would indeed be difficult to afcertain with any degree of precifion, the quantity neceffary for maintaining a common fire, but if the combuftion of a middle fized candle be equal to the confumption of one perfon by refpiration, it is certainly much under-rating the quantity if taken at four times that of the former. However, fuppofing it to amount to no more, it muft follow that the oxigen gas decompofed by refpiration and combuftion only, in the City of London, amounts to the enormous quantity of five million cubic feet *per* hour.

Great importance and prodigious confumption of oxigen in the fyftem of nature.

Confumption in London 120 millions of cubic feet in a day.

Soon after the difcovery of the component parts of atmofpheric air, Drs. Prieftley and Ingenhouz fhewed by a great number of experiments, that oxigen gas was emitted by vegetables when expofed to the rays of the fun. From this fact it was immediately concluded, that the atmofphere was principally, if not altogether, fupplied with oxigen gas from this fource. This opinion, which ftill obtains without any attempt to controvert it, appears to me objectionable in feveral refpects. In the firft place, it is doubtful if the fupply from this quarter be at all adequate to the confumption for Dr. Prieftley, likewife found that vegetables during the night, emitted a gas totally

Renovation of the air by vegetables;

ſuppoſed to be inſufficient for reaſons ſtated.

tally unfit for reſpiration or combuſtion, ſo that the oxigen gas formed in the day would not only be conſumed in the night by theſe proceſſes, but a deleterious gas added to the atmoſphere. But ſuppoſing the quantity of this latter to be ſo ſmall as to produce no effect, yet it muſt be admitted, that while the ſun is under the horizon, none of the former can be produced; indeed it is only by the direct influence of the ſolar rays on living vegetables that it is evolved at all. How many circumſtances then under the temporary deprivation of light, muſt concur to obſtruct the evolution of this ſalutary fluid from vegetation. The inhabitants of countries where fogs or clouds obſtruct the ſolar rays for weeks, nay months together, muſt be indebted for life to the winds wafting oxigen gas from regions far diſtant from their own, where a perpetual ſpring and cloudleſs ſkies prevail. In the depth of winter, when half the world lies buried under ice and ſnow, and vegetation ſuffers a temporary ſuſpenſion, every living creature in theſe climates muſt be indebted to the ſame friendly winds and regions for health and life.

Oxigen abounds no leſs in the air over ſeas, deſerts, and frozen regions, as in places where vegetation and the ſolar influence predominate the moſt.

Thoſe portions of the ſurface of the globe which furniſh oxigen gas from vegetation, are not only ſubject to moſt of the perpetual interruptions already mentioned, but make but a ſmall part when compared with the whole. The vaſt tracts of ocean can afford nothing for the preſervation of animated beings more than the ſandy deſert bounded only by the horizon, or the eternally frozen regions of either pole, and the blaſts of the north would carry deſtruction to all at leaſt who inhabit near the artic circle. It would be only in the torrid zone where we could expect to find, in ſufficient quantity, that life ſupporting fluid, which, unfortunately for the common received opinion, is as abundant in the midſt of London at all times, as at Nova Zembla, or on the line. As nature, ever uniform, makes the moſt ample proviſion for carrying on her operations throughout her works, it cannot be ſeriouſly maintained that ſhe would truſt a buſineſs of ſo much importance as the preſervation of animal life, to means ſo precarious as clear or cloudy weather, or the viciſſitudes of the ſeaſons. The quantity of oxigen gas contained in the atmoſphere is found to be nearly the ſame at all times and in all places, whether in the depth of winter or middle of ſummer, whether on land or on water, whether in the crouded city or remote hamlet, but if the

the general opinion were juſt, it would be ſubject to perpetual variation. From this ſingle fact we infer, that affinity or attraction is concerned in keeping up a conſtant and regular ſupply, and that however unequal at different times the conſumption may be, that power will ſtill continue to act in a proportionate degree. Several years ago, I conjectured that the water in a ſtate of ſolution in the atmoſphere, or that which compoſes the clouds, might be decompoſed by the action of light, and conſequently furniſh a ſufficient ſupply. With this view I made ſeveral experiments, by expoſing water in glaſs veſſels to the influence of the rays of the ſun, but could never ſucceed in producing a ſingle bubble of any ſort of gas, although confined with air of different degrees of denſity, and in vacuo, and expoſed for ſeveral months. At that time I attributed the want of ſucceſs to the difficulty of imitating nature, therefore did not abandon the idea entirely.

The ſupply is probably by direct chemical affinity;

not perhaps from decompoſition of aqueous vapor,

Meditating on the ſubject ſome time after, I conceived it extremely probable, whatever may have been alledged to the contrary, that azotic gas has ſome degree of affinity for oxigen, and that the combined action of this fluid and light might be ſufficiently powerful to decompoſe water. To determine this point, I made the following experiment, which ſucceeded beyond expectation. Into a ſmall tranſparent glaſs retort, filled with freſh drawn pump-water, I introduced 17 ounce meaſures of pure azotic gas of the temperature of 56°, and inverted it into a bottle filled with water, and ſunk to the neck in a pot of ſand; the mouth of the retort deſcended to within about an inch of the bottom of the bottle. After accurately marking that part of the neck of the retort at which the water ſtood, it was placed on the outſide of a window fronting the ſouth, April 22, 1801. On the firſt three or four days of expoſure, a number of very minute bubbles appeared on the ſides of the glaſs and roſe to the ſurface. They after this entirely ceaſed, and the water became ſomewhat turbid. In about three weeks, having recovered in a great degree its tranſparency, an infinite number of ſmall yellowiſh particles were uniformly diffuſed through it, and which continued to the end of the experiment. The volume of included gas was perceived to be increaſed ſome time after expoſure, *and after ſome days of uninterrupted ſunſhine*. Some time in the beginning of October, long after the gas had ceaſed to gain any additional bulk,

but from the diſpoſition of azote to unite with oxigen.

Experiment. Azote gas was expoſed to water in an inverted veſſel.

In the courfe of time the gas was increafed,

bulk, the apparatus was taken in, and being brought to the temperature of 56°, I had the fatisfaction to find it was increafed confiderably. After marking the neck of the retort where the water now ftood, in order to know whether any farther change would take place, the whole was placed in a *dark* clofet, near which a conftant fire was kept. After ftanding till November 23, 1802, no increafe or diminution had taken place. Upon examination, the increafe was found to be fomewhat more than *four ounces*, as the whole meafured upwards of 21. It now exhibited all the properties of *atmofpheric air*. A fmall taper continued to burn in a given portion the fame fpace of time as in an equal quantity of common air. By feveral trials with Mr. Davy's eudiometer, it was found to contain a fomewhat larger proportion of oxigen gas then the air of a large room where the trials were made. And the teft by fulphuret of potafh fhewed the fame. What appeared furprifing was, that no hidrogen gas could be found. Its abfence is not eafily accounted for, I can only conjecture that the hidrogen might have entered into combination with fome extraneous fubftance contained in the water, as the oxigen was evolved particularly, as all that part of the infide of the retort with which the water was in contact, was lined with a pelucid whitifh film, which came off in large flakes upon rinfing it. It feems to me extremely difficult if not impoffible, to account for the oxigen gas produced in this experiment, otherwife than from the decompofition of the water. It cannot be fuppofed that this quantity of gas (no lefs than 4ozs.) was held in an uncombined ftate in the water, which in all amounted to little more than a pint. That portion of oxigen gas which is found naturally in water, feems to have been extricated at the beginning of the experiment as already mentioned, which altogether could not have amounted to two drams by meafure at moft, and no more was obferved to arife. Neither could it be from putrefaction, as the water was free from fmell, and no hidrogen gas produced.

almoft one fourth,

and was better than common air.

There was no hidrogen.

But the oxigen is neverthelefs thought to have come from the water.

Qu. If the developement of oxigen from water in contact with azote be not the renovating caufe in the atmofphere.

Are we to conclude, then, from thefe facts, that by the combined action of light and azotic gas water is decompofed, and that when the gas ceafes to act, or is faturated, if I may ufe the expreffion, no further decompofition takes place till a portion of the oxigen gas is feparated? If this be admitted, we can readily account for the immenfe quantity of oxigen gas

with

with which the atmofphere muft be neceffarily fupplied to keep pace with the confumption, and alfo why it is only found in one invariable proportion.

VII.

*Report on a Memoir * fent to la Societé Libre des Pharmaciens, of Paris. By Cit.* ROBERT, *Chief Phyfician to the Hofpital of Humanity, at Rouen. By † Citizens* C. L. CADET and BOULLAY.

CITIZEN ROBERT recounts the different experiments made by Citizens Fourcroy and Vauquelin, on the effects of fur-oxigenated muriate of potafh united to certain combuftible bodies, and fubmitted to the action of a blow or friction; thefe brilliant detonations are now known to all chemifts; they are equally well informed of the inflammations which take place when the fame bodies are plunged into concentrated fulphuric acid. Cit. Robert has modified the latter experiments in an interefting manner. Inftead of throwing the inflammable mixtures into the acid, he only touches them with a tube wetted with this acid. In this manner he inflamed,

Introductory obfervations.

Cit. Robert's experiments.

1ft. Three parts of the fur-oxigenated muriate, and one part of fulphur.

2d. Three parts of the fame falt, half a part of charcoal, and as much fulphur.

3d. Equal parts of antimony and fur-oxigenated muriate.

4th. Equal parts of fulphuret of antimony and of the falt.

5th. Equal parts of kermes and golden fulphur (and the falt.)

6th. Equal parts of arfenic and the falt.

7th. Three parts of the muriate and one of fugar.

8th. Three parts of the muriate and one of charcoal.

9th. A part and a half of the muriate and three parts of gunpowder.

* The fubject of the Memoir itfelf is the inflammation of combuftible bodies combined with fur-oxigenated muriate of potafh, by contact with fulphuric acid. C & B.

† From the Annales de Chimie, Frimaire, An. XI.

And

And laſtly, paſtes made with alcohol, olive-oil, and ſur-oxigenated muriate of potaſh.

repeated with ſucceſs;

We have repeated all theſe experiments which ſucceeded perfectly, as well as the following, which appeared with reaſon to Cit. Robert, to deſerve attention.

He charged a piſtol with common gunpowder, and having primed it with the mixture of powder and muriate mentioned above, he inflamed it with the ſulphuric match *, and it went off.

further experiments requiſite to complete the enquiry.

Cit. Robert obſerved, that the report, the colour of the flame, and the odour exhaled by the various combuſtibles are eſſentially different; but he made no enquiries reſpecting the gaſes they formed, or the compounds which remained after the inflammation. We propoſed to collect the gazeous products in a hidro-pneumatic apparatus, for the purpoſe of examining them, but the time appointed by the ſociety for making our report being but a few days, we have poſtponed theſe enquiries.

Tho' curioſity alone may appear intereſted in purſuing ſuch experiments, we have thought it a duty to try, in a ſimilar manner, ſeveral ſimple and compound ſubſtances, the inflammation of which had not been before attempted.

New Experiments made by the Commiſſioners.

New experiments.

The manner of operating being nearly the ſame, to avoid the continual repetition of the ſame formula, we ſhall only point out the ſubſtances united with the ſur-oxigenated muriate of potaſh. Phoſphorus preſented beautiful deflagration. Hidrogen gas took fire. To perform this experiment, we filled a bladder with inflammable air. To its ſtop-cock we affixed a ſimple adjutage or copper tube with a ſingle jet. We moiſtened this tube with ſulphuric acid, and touched it with ſur-oxigenated muriate of potaſh, at the ſame time preſſing the bladder to make the current of the gas paſs over the ſalt. At the inſtant of contact the gas took fire, as if by the electric ſpark.

Phoſphorus. Hidrogen gas.

Metals.

Gold, ſilver, zinc and iron, yielded no other phenomenon but the decrepitation of the ſalt alone. We were not ſurpriſed

* The tube and ſulphuric acid. Tranſlator.

at

at this negative refult with the two firft metals, which are not eafily oxidable, but zinc and iron promifed an inflammation, becaufe they detonate by a blow.

Metallic oxides and fulphurets.

Brown oxide of copper, the refidue of the diftillation of acetate of copper burnt without flame, with fparks refembling thofe from *gerbes d'artifice* (a kind of firework called the wheat fheaf.)

Metallic fulphurets fucceeded pretty well, particularly the fulphuret of tin or aurum mufivium, and the black fulphure of mercury. The laft yields a very beautiful white flame. Amber, fuccinic acid, bitumen, did not take fire, but the decrepitation was very confiderable.

Vegetables.

Among the vegetable fubftances fome inflamed eafily, fuch as the volatile oils, refin, turpentine, gum copal *, and gum elemi, gum arabic, the duft of lycopodium, foap, camphor, cotton, and fawduft. This laft article did not always fucceed, but with the addition of a little fulphur it makes an excellent powder of fufion, which from the facility of its inflammation by a re-agent, may be ufeful to mineralogifts.

We tried ftarch, it inflamed with difficulty, but we fucceeded in making it burn. Ether takes fire very quickly. We obferved that in this experiment as well as in thofe with camphor and alcohol, very little of the fur-oxigenated muriate was decompofed, and it did not burn, but was folely of fervice to favour the inflammation. To afcertain that fact, we mixed fulphur with the refidue of the experiment, the fulphuric match produced a fecond combuftion.

We made a pafte with the fur-oxigenated muriate and honey; this mixture caught fire, fwelling up and emitting an odour of boiling fugar mixed with a very penetrating acid, which one of us believed to be acetic acid.

Chryftallized benzoic acid diffufes a confiderable reddifh flame; tartareous acid and acidule burn alfo very well; tartrite of potafh offers a beautiful whitifh flame; tartrite of foda prefents neither inflammation nor light. Antimoniated tartrite of potafh gives beautiful fparks without flame.

Oxalic acid with the oxigenated muriate fparkles without inflammation.

* The gum refin, copal, ftill but little known, has been affimilated by fome chemifts with amber. This experiment eftablifhes a very remarkable difference between thefe two fubftances.

inflammation. Acetic acid produces a confiderable deflagration and a beautiful blueifh flame. The acetates of potafh and foda take fire with crackling.

Animal matters. Thefe refults induced us to examine what animal matters were fufceptible of burning by the fame proceffes; we tried dried gluten and hartfhorn fhavings without fuccefs.

The yolk of an egg, wax, butter, tallow and greafe, burnt like oil, but with lefs crackling; wool and a piece of rabbit's fkin with its fur on, ftrongly impregnated with fur-oxigenated muriate of potafh, were inflamed and continued to burn until their total incineration.

Singular refults. Among thofe experiments which prefented us remarkable fingularities, we muft mention the fulminating powder which we could never fet fire to, though it was decompofed with a confiderable difengagement of gas and heat. Three other mixtures with metallic bafes furprifed us greatly by their terrifying detonation, by their rapid inflammation, and by their ftrong effect in fire-arms*. Cit. Robert has no doubt tried them, but he makes no obfervation on them. We fhall copy his referve, and only allow ourfelves two remarks. The arms are ftrongly oxided and foon deftroyed by thefe mixtures, which are expenfive and more difficult to prepare than gunpowder; we think they are not capable of being granulated. Their inflammability would render their carriage and their ufe too dangerous, fince a blow or friction produces detonation. We fhall conceal their names, becaufe malevolence too often abufes the fatal fecrets made known by chemifts. The remembrance of the calamities at Effone, ftill more the public fafety makes it our duty to be filent, and not to give a new refource to the cruel art of deftroying mankind.

* They take fire like powder by the fpark from a gun flint.

Defcription

VIII.

Deſcription of a Telegraph uſed in Sweden. Conſtructed by Sir A. N. EDELCRANTZ, *Counſellor of the Chancery, and private Secretary to the King of Sweden, Archiviſt of the Orders of His Majeſty, and One of the Eighteen Members of the Swediſh Academy. From the Copy of a Treatiſe on Telegraphs, communicated by the Author.*

AFTER numerous obſervations on the various deſcriptions of ſignals, and an hiſtorical detail of the uſe of them, from the moſt remote antiquity to the preſent time, the author ſtates the principal qualities of a good telegraph to conſiſt in: Qualities requiſite in a telegraph;

1. Perfection in the principle of the machine, and in the nature of the ſignals.

2. Perfection in the means employed to work the machine.

The firſt of theſe requires: 1. a ſufficient number of ſignals to expreſs not only letters, but alſo ſuch words and phraſes as are in general uſe: 2. that the ſignals ſhall be ſo apparent, that any one may ſee them very diſtinctly: 3. that the application of the ſignals to their reſpective ſignifications ſhall be eaſily underſtood and retained.

The ſecond requires: 1. the leaſt poſſible weight and friction of the parts: 2. quickneſs in the movements and changes of the ſignals: 3. certainty to prevent miſtakes and ambiguous reſults.

The credit of uniting all theſe qualities in one machine, are aſcribed to M. Chappe, the inventor of the French telegraph, of which he gives a deſcription. The number of ſignals it is capable of, allowing the angle of inclination of its reſpective arms to be 45°, is 256; by diminiſhing that angle to 30° they may be increaſed to 864, and in like manner the number may be ſtill further augmented by every diminution of the angle, but the ſignals would be proportionally leſs diſtinct. firſt united by M. Chappe.

When this diſcovery was announced, the learned of all countries inſtituted enquiries into its principles and conſtruction. Among others, the Chevalier made various machines, ſome reſembling the French telegraph, and others totally different, and after many attempts he ſucceeded in conſtructing that which is the principal ſubject of his treatiſe, and to which he Attempts to diſcover the conſtruction and principles of the French telegraph.

Advantages of that difcovered by the author.

gave the preference, becaufe it occupied lefs fpace, was more rapid and eafy in its movements, more certain and unequivocal to the fight, and, to all its other advantages, added that of being ufeful by night.

Defcription of the Machine.

Defcription.

a, b, c, d, e, f, (Fig. 1, Pl. XII.) is a frame of wood, in the openings of which are placed ten fhutters at equal diftances from each other, in three vertical ranks, that in the middle confifting of four fhutters. Thefe fhutters, which fhould be as thin as poffible, may be made of wood, iron or copper, and are fixed on an axis *o n,* the extremities of which move in holes made in the frame. On the axis *o n,* perpendicular to its direction, but with an angle of 45° to the furface of the fhutter is a fmall arm *m s,* from one extremity of which a ftring defcends to the bottom of the frame, by means of this and of the arm *m s,* the fhutter receives at pleafure a vertical or horizontal pofition. Fig. 1 reprefents all the fhutters in a vertical pofition, and Fig. 4 fhows the profile of the machine, the fhutters being horizontal. It will be eafily feen, that when the fhutters which are very thin, are obferved from a diftance and horizontally, they will be fcarcely perceptible; but they become vifible, if, by drawing the ftring *s k,* a vertical pofition is given to them. When the machine is at reft, the fhutters are kept in a horizontal pofition by a weight *q* attached to them at *l,* and are fupported by the pin *p l.* In the other pofition they are kept upright by a piece of wood *p* place behind them, but which is fhown in front in the plate. To prevent the ftrings from getting entangled, the arms *m s* are fixed at unequal diftances from their refpective fhutters. The ftrings pafs through holes made in the crofs plank *u r,* or *u u u,* (Fig 2.) which fhows the plan. They may then be made to pafs through the plank *w w,* and be fecured to ten rings *t t,* each correfponding to one finger, by which means a fmall machine may be conveniently worked.

Number of its fignals.

According to this conftruction, 1024 combinations or diftinct fignals may be formed by raifing or lowering the fhutters. The better to diftinguifh them they are expreffed by numbers on the following principle. Leaving out the upper fhutter A, the machine will confift of three vertical lines, with three fhutters on each. To the upper fhutter of each line give the value

 of

of 1, to the ſecond 2, and to the laſt 4, and from the value of the ſingle ſhutters form that of their combinations. Thus when the firſt and ſecond ſhutters on a line are viſible, they will ſignify $3 = 1 + 2$, the firſt and third $5 = 1 + 4$, the ſecond and third $6 = 2 + 4$, and the three together $7 = 1 + 2 + 4$, and in the ſame manner for each vertical line. If in any of the lines no ſhutter is viſible, the vacancy is expreſſed by o. Theſe three lines give 512 different ſignals, and the number is doubled by the addition of the ſhutter A. Thus in expreſſing the number of the ſignal, ſuppoſe the firſt and ſecond ſhutters of the firſt line to be ſeen, all three on the ſecond line, and none on the third, the value muſt be written 370, if to theſe the ſhutter A is added, the number muſt be expreſſed by A 370, and ſo of every other combination. On the left ſide of the machine is placed an arm H, the uſe of which is partly to direct the arrangement of the lines, beginning from that ſide; and partly to point out the middle ſhutter in each line. It may be made moveable on hinges, and can then be concealed or made viſible at pleaſure. It will be eaſily conceived that the method of valuing the lines may be varied by agreement, ſo as to be reckoned from right to left, or horizontally, in either direction: thus the poſition of the telegraph as indicated above, may be expreſſed by 370 — 670 — 073 — 076 — 662 — 266 — 332 or 233, but the method firſt deſcribed ſeems to be the moſt natural.

By the mode of reckoning, the value of the ſhutters as deſcribed above, it will be ſeen that they are capable of expreſſing every number of three places, from 000 to 777, which does not contain an eight or nine. To make uſe of theſe numbers, a table muſt be conſtructed on the following plan,

Table of cyphers.

000	100	200 and fo on	to 700
000 - - - 777	100 - - - 677	200 - - - 577	
001 - - - 776	101 - - - 676	201 - - - 576	
002 - - - 775	102 - - - 675	202 - - - 575	
003 - - - 774	103 - - - 674	203 - - - 574	
004 - - - 773	104 - - - 673	204 - - - 573	
005 - - - 772	105 - - - 672	205 - - - 572	
006 - - - 771	106 - - - 671	206 - - - 571	
007 - - - 770	107 - - - 670	207 - - - 570	
010 - - - 767	110 - - - 667	210 - - - 567	
011 - - - 766	111 - - - 666	211 - - - 566	
012 - - - 765	112 - - - 665	212 - - - 565	
013 - - - 764	113 - - - 664	213 - - - 564	
014 - - - 763	114 - - - 663	214 - - - 563	
015 - - - 762	115 - - - 662	215 - - - 562	
016 - - - 761	116 - - - 661	216 - - - 561	
017 - - - 760	117 - - - 660	217 - - - 560	
020 - - - 757	120 - - - 657	220 - - - 557	
021 - - - 756	121 - - - 656	221 - - - 556	
022 - - - 755	122 - - - 655	222 - - - 555	
023 - - - 754	123 - - - 654	223 - - - 554	
024 - - - 753	124 - - - 653	224 - - - 553	
025 - - - 752	125 - - - 652	225 - - - 552	
026 - - - 751	126 - - - 651	226 - - - 551	
027 - - - 750	127 - - - 650	227 - - - 550	

completing the feries in each direction until the whole number of combinations are expreffed, and filling up the vacancies between the double row of numbers with fuch letters, figures, words or fentences, as may have been previoufly agreed upon.

its ufe by night.

The two rows of numbers are only neceffary when the telegraph is intended to be ufed by night as well as by day, in which cafe it muft be obvious, that when the concerted fignal is made by raifing the fhutters, thofe lights which are placed behind them, as will be defcribed hereafter, become hid, and the value of the fignal is reverfed in confequence of the open fpaces being vifible inftead of the clofed ones.

A fecond table muft alfo be formed to contain the fame feries with the letter A prefixed as defcribed above.

The correfpondence may be fecret.

When the correfpondence requires fecrecy, the fignifications of thefe numbers, and the methods of eftimating their value may be varied almoft to infinity by any one verfed in the knowldge

knowledge of cryptography. Several plans of this kind are inferted by the Chevalier, the details of which would extend the limits of this paper too far.

Methods of working the Machine.

It has been already noticed, that a fmall machine may be worked by the rings *t t*, but as the ftrength of the fingers are unequal to the weight and refiftançe of a large machine, more powerful means muft be had recourfe to. With this view the author propofes to replace the rings with metal rods affixed to the ftrings, and having a button at the lower extremity of each. To work the machine fo prepared, he propofes to have an inftrument of iron nearly in the form of a T, at the bottom of which fhall be an opening or ftirrup to receive the foot. The crofs arm of it is to have ten notches correfponding to the ends of the rods, and is to be kept on a level with them by a weight behind. When a fignal is to be made, the ends of the rods hanging to the fhutters which are to be clofed, are flipped into their refpective notches, and the machine is worked by preffing down the inftrument with the foot. When the fignal has been obferved the foot is withdrawn, and the inftrument reftored to its original pofition, by the weight which has been defcribed. The rods are then difengaged, and the whole is in a ftate of preparation for the next fignal. This may be more expeditioufly performed by another contrivance formed of ten plates of metal moveable on a common axis, and each having a hollow in its outer edge. When the machine is at reft, thefe plates are in a vertical pofition, and the box containing them is placed before the lower ends of the rods. To make a fignal, the plates correfponding with the rods intended to be acted on, are pufhed with the fingers into an horizontal pofition, and the box flid along in two grooves made for the purpofe, by which means all thofe rods are preffed into their refpective notches at once, and a confiderable fpace of time is faved, while the rods whofe correfponding plates retain their vertical pofition are unmoved. If a ftill greater force is required to work the machine than can be obtained by the foot, a winch with rackwork, or with a pulley, inftead of the upright bar of the inftrument, may be applied to the crofs arm.

Methods of working.

In order to afford more perfpicuity in making a fignal, the Chevalier alfo propofes to form the combinations of each re-

Each fignal may be made by drawing one rod on each line.

fpective

ſpective line by means of united ſtrings, thus to repreſent 1, a ſtring attached to that ſhutter is faſtened to a rod numbered 1; for 2, a ſtring is brought from that ſhutter to a rod numbered 2; but to produce 3, a ſtring from each of the ſhutters 1 and 2 is affixed to the rod numbered 3, and in a ſimilar manner 5, 6 and 7, are each produced by combining the ſtrings before they are united to the rods. In this method the number of the rods are increaſed to 22, but as only one of each line can be uſed at once, and they are all numbered, there is leſs probability of a miſtake, and the quickneſs of the operation is increaſed.

Requires only one or two perſons to work it,

According to the conſtruction above deſcribed, it will be ſeen that one perſon is ſufficient to work the largeſt telegraph at the extremities of the line of ſtations, provided the teleſcope is ſo placed that he can look-out and work the ſhutters at the ſame time; but in the intermediate ſtations, where the obſervations muſt be made with two teleſcopes, in contrary directions, there ſhould always be two perſons who may relieve each other.

who need not be men of ſcience.

It is not requiſite that they ſhould be men above the common rank; all that is wanted of them is to be able to write down the numbers, and to combine 1, 2, 3, 4.

Children employed.

In the attempts made by the Chevalier, he, during a year and half, employed only children, and a very few hours were ſufficient to give them a complete knowledge of what was required of them. It was for this reaſon he was induced to ſacrifice ſomething of the ſimplicity of the mechaniſm to ſimplicity in the execution. When thoſe employed are not well ſkilled in the uſe of the machine, it is important not to load their memory or their conception with too many details, and it is probably to this as much as to rapidity of execution that the chief merit of this telegraph is owing.

The obſervatory.

The obſervatory ſhould always be placed below the telegraph, and ſhould encloſe the teleſcopes and machinery with which the telegraph is worked. It ought to be as much darkened as poſſible, and painted black withinſide, in order that all foreign light may be excluded, by which means the impreſſion of an object is rendered much more diſtinct to the eye.

Methods of uſing the Telegraph by Night.

Methods of uſing the telegraph by night.

For the purpoſe of rendering the telegraph uſeful in the night, the following plans are adopted.

In

In the firſt it is propoſed to place a lamp behind each ſhutter at ſuch a diſtance as not to obſtruct the movements, and rather above the axis. In this method, by cloſing the ſhutters according to the plan indicated above, the lamps correſponding to the ſhutters which are raiſed will diſappear, and thoſe which remain viſible will form a ſignal the reverſe of that intended, the value of which will be found on the right ſide of the table of cyphers deſcribed before; hence the day ſignal will be before the ſignification, and the night ſignal after it. But as this plan was found to be inconvenient in practice, or at great diſtances the following was ſubſtituted for it, *a, b, c, d,* (*Fig.* 3.) is a tin lanthorn having only two openings *e,* to permit the paſſage of the light; placed on oppoſite ſides, and covered with talc or muſcovy glaſs. Between theſe two openings a good lamp is ſituated ſo as to give light in both directions. *g, f, k* is a quadrant of tin adapted to each ſide of the lanthorn, and moveable on its axis *h,* in ſuch a manner, that by means of a ſtring attached to the arm *i,* it may be raiſed before the openings of the lanthorn, but falls again by its own weight when the ſtring is let go. Ten of theſe lamps are to be fixed on a frame in a ſimilar manner to the ten ſhutters uſed by day, the ſtrings depending from each are united at the foot of the machine, according to the ſame principles and the ſame combinations as in the day telegraph; but as the weight and friction to be overcome is much leſs, this machine may be worked at all times with the hands alone. One advantage which this eſtabliſhment poſſeſſes over the day telegraph is, that it does not require to be placed above the horizon, it is even better to be below it. Beſides, in every point of view it is more eaſily worked and is much ſuperior in utility to the telegraph.

First plan.

Second plan.

It is known that the ſtars may be ſeen in the middle of the day from the bottom of a deep well; it remains to be proved, if, by a ſimilar effect the lamps can be ſeen in the day-time with the aſſiſtance of very long tubes: ſhould this be practicable it will be highly advantageous in many reſpects, particularly in requiring only one eſtabliſhment for both purpoſes. By means of the equatorial teleſcopes of Short or Ramſden, the ſtars may be obſerved at noon even cloſe to the ſun, hence it is probable that if all foreign light is excluded, the lamps will be viſible on a dark ground. Teleſcopes which magnify the moſt will be preferable, becauſe they darken the field of view

Probable viſibility of the lamps by day.

view more than thofe of fmaller power, without diminifhing the impreffion of the light on the eye in any confiderable degree.

Ufefulnefs of telegraphs,

Opinions are divided on the utility of telegraphs, and fome of their opponents found their arguments on the alterations to which the tranfparency of the air is liable, and which impede their ufe. But the art of feeing through fogs and darknefs will perhaps never be difcovered. It will fcarcely be contended that navigation is a ufelefs fcience, becaufe winds and tempefts fometimes impede it. Others confider the expence of telegraphs as too great in proportion to their utility.

to governments.

But independent of the advantages arifing to every government, in being enabled to tranfmit its orders to the extremities of the kingdom in lefs than a quarter of an hour, and to be no lefs expeditioufly informed of what is paffing on the frontiers, it is in time of war, when events occur on the knowledge of which the fafety of thoufands may depend, and which require precautions as prompt as vigorous; it is in fuch a period that the value of telegraphs is incalculable, and their coft bears no comparifon with their utility, particularly if the expence of couriers faved by their means is taken into the account. It may be added, that the ufe of thefe machines in France during the late war furnifhed inconteftable proofs of their utility.

There are two ways in which this machine may be ufeful; the firft, when a quick communication between the two places is required; and the fecond, when this communication is not practicable in any other way. The poft goes from Sweden to Finland in five days, but if the diftance was divided into telegraphic ftations, intelligence might be conveyed in three or four minutes. Should the poft be retarded or interrupted either by the obftacles of the feafon, by the infecurity of the lakes, or by contrary winds or tempefts, the communication may be kept up with the cuftomary expedition of the poft, by forming ftations in thofe places where the principal obftructions are met with, fuch for example as the fea of Aland; or with the greateft rapidity, by placing them along the whole road.

to fcience.

From every appearance it is expected that natural philofophy will acquire many important benefits from thefe eftablifhments. Every telegraph is a real obfervatory, which, with the affiftance of fome inftrument, and the requifite information in

in thofe who govern it, might enrich the fcience with new difcoveries. An acquaintance with the effects of the refraction of the air might be diffeminated by daily experiments; meteorology, which is the leaft cultivated, and perhaps the moft difficult branch of natural philofophy, might be ftored with difcoveries on the influence of heat and cold, humidity, ftorms, changes of weather, fogs, fnow and rain, as well as that produced by feveral other meteors on the tranfparency of the air; a daily comparifon of the indications given by the barometer, the hygrometer, the anemometer, and the electrometer, with thofe of the diaphanometer of M. de Sauffure *, and of the cyanometer †, might, in time, lead to very unexpected refults.

At the conclufion of the work, the chevalier notices the telegraph erected on our admiralty, which appears to have been an invention fubfequent to his, and to be lefs complete. His telegraphs were eftablifhed in Sweden in 1794, ours was not erected until 1796, and the number of its fignals does not exceed 64, unlefs thofe are reckoned which depend on the order in which the fhutters are fhown, and which are liable to great ambiguity. English telegraph.

IX.

Additional Obfervations on the Probability that the Eruptions of Lunar Volcanos may fometimes reach the Earth. By a Correfpondent.

To Mr. NICHOLSON.

SIR,

SOME time ago, I addreffed a letter to you, *on the probability that the eruptions of lunar volcanos may fometimes reach the earth*; which was inferted in your Journal for December laft. I will now beg leave to trouble you with a few additional obfervations on the hypothefis, I there ventured to advance.

* *Effay fur l'Hygrometrie*, § 371.

† See a defcription of this inftrument in obfervations fur la phifique et fur l'Hiftorie Naturelle, par Rofier, 1791, mars, p. 199.

In

More particular illuſtration of the probability of projectiles falling from the earth to the moon.

In Fig. 4. Plate X. ſuppoſing E and M to be the centers of the earth and moon; P Q K to be a path which a body muſt deſcribe, ſo as to be equally attracted by both; it will appear evident from a very eaſy calculation, that, P Q K is a circle whoſe radius C P is equal to about $24\frac{1}{2}$ ſemidiameters of the moon, and the diſtance of whoſe center C, from the center of the moon, is equal $2\frac{1}{3}$ ſemidiameters (nearly). If therefore, the lunar volcanos in any part of the hemiſphere of that planet, which is viſible to us, ſhould project bodies with a force ſufficient to carry them through 24 of her ſemidiameters; i. e. with a velocity of about * 7000 feet in a ſecond, they muſt neceſſarily throw them within the ſphere of the earth's attraction. It may be ſaid, however, that the atmoſphere of the moon, although rarer and of leſs altitude than that of the earth, may yet be conſiderable enough to afford ſome reſiſtance to the motion of bodies; allowing, therefore, (the utmoſt that can be allowed) that a body projected from a lunar volcano, has a reſiſtance equivalent to that of two miles of an atmoſphere of equal denſity with ours, and ſuppoſing the velocity of projection to be 12000 feet per ſecond, and the body to be a ſphere whoſe diameter is one foot, and ſpecific gravity 10,000 times that of the atmoſphere; it would loſe in its paſſage leſs than $\frac{1}{3}$ of its firſt velocity, and would ſtill retain more than ſufficient force to carry it within the ſphere of the earth's attraction.

Terreſtrial volcanos do produce the requiſite velocity.

That the volcanos of the earth throw out heavy bodies with a force at leaſt equal to this, ſeems eaſily proved; for a body ſuch as we have ſuppoſed, if projected upwards with the above-mentioned velocity, would riſe to the height of about nine miles from the earth's ſurface, in a medium of equal reſiſtance with our atmoſphere. It may be objected, that the atmoſphere becomes conſiderably rarer at that height, ſo as to render the calculation of the effect of reſiſtance greater than it would be in reality; this, however, may be more than counterbalanced by the effect of the elaſticity of the atmoſphere, and of the vacuum produced by bodies moving with ſuch very great velocity. That volcanos frequently during an eruption, throw up heavy bodies to the height of eight or nine miles, will, I believe, be acknow-

* It was by miſtake that I ſtated in my laſt letter, the velocity neceſſary at 12,000 feet per ſecond.

ledged.

ledged. I will now conclude with an attempt to anſwer your objection, from the bodies being in a ſtate of ignition when they fall to the earth. As the ſpace between the earth and moon muſt be either nearly, or altogether a vacuum, it muſt be almoſt, if not quite, a non-conductor of heat; ſo that it may eaſily be conceived, that a body paſſing through it, may retain during its paſſage of about four or five days, nearly the ſame degree of heat with which it ſet out, eſpecially as no change of texture takes place, by which its heat can become latent.

I remain,

Your's, &c.

Cambridge, May, 24th, 1803. J. B.

P. S. For the principles of the eſtimations, which I have made of the reſiſtance of the atmoſphere. See *Atwood on Rectilinear Motion,* ſect. 5th.

X.

Deſcription of an Ancient Lock of Combination. W. N.

THE lock delineated in Plate IX. is fixed in the lid of an old braſs money box for the pocket; from which Mr. Latham of King Street, Soho, who deals in articles of curioſity, permitted me to make a drawing.

Secret lock of an old money box.

Fig. 1. is a perſpective view of the box, which meaſures about three inches in diameter. Fig. 2. ſhews the outſide of the lid, at one edge of which is a hinge, and upon the outſide are figures of the ſun, the moon, and two clock faces with hands. The figure of the moon can be ſlided nearer or farther from the center, and carries a bolt placed within the lid. The other three parts, namely, the ſun and the hands of the clock, can be moved round their center pieces, and left in any ſituation at pleaſure. Fig. 3. ſhews the inſide of the lid. The bolt which is moveable along by means of the figure of the moon, is here ſeen with three tails, pointing to three circular pieces, in each of which pieces there is a notch to receive its correſpondent tail. Theſe pieces move round by means of the three parts on the other ſide, and it will thence be eaſily underſtood that the bolt cannot be ſlided back, nor the box

box opened, unleſs one particular point of the ſun, and each of the hands be duly placed from a knowledge of the ſecret. And as there are twelve points to the ſun, and twelve marks for the hours on each dial, we may (without conſidering the probability of intermediate poſitions) reckons the chances as eleven to one, againſt ſetting the ſun to the proper poſition; and eleven times ſtill more againſt ſetting the ſun and face of the dials right at the ſame time; and again eleven times more againſt all theſe being duly ſet at once. That is to ſay, the chances are 11 times 11 = 121, taken 11 times or = 1331. And conſequently the lock is very ſecure.

The ſecret of the lock is ſecure as 1331 to 1.

As I hope ſoon to give a ſhort eſſay on locks, I ſhall only make a few remarks on the preſent machine. In the firſt place, it may be noticed that this, like the lock of Cardan*, which conſiſts of four or more rollers, is a ſecret lock, and not a lock of combination; this laſt term being uſually applied to locks in which a ſecret can be changed through a great number of varieties, at the pleaſure of the owner. And, ſecondly, that it has a practical defect very common to all ſecret locks; namely, that a ſkilful examiner may diſcover the ſecret, if the workmanſhip be in the ſmalleſt degree inaccurate. In this manner I opened the preſent lock, at the very firſt trial. If the three notched pieces be not truly circular, and the motion of the bolt extremely preciſe, and the lengths of the tails accurately adjuſted, one of theſe tails will bear before the other. I therefore preſſed the bolt back, by urging the figure of the moon towards that of the ſun, and then turned the three pieces round one after the other. Two of them moved freely round, and the third gave notice when the bolt fell in, and conſequently told its ſecret. Of the remaining two pieces, one moved freely, and the other hung and ſhewed its poſition. And laſtly, the third then required only to be moved till the bolt took its notch, and went quite back.

Cardan's lock.

Secret locks and locks of combination.

The preſent lock was eaſily opened.

This lock could be eaſily made ſafe, by adding ſhort teeth to the inſide pieces all round, and making the tails of the bolt terminate in ſmall projections adapted to take between thoſe teeth. By this contrivance, the tentative proceſs of turning the pieces would be rendered impracticable.

But it may be made ſafe by a very ſlight change.

* The ſecret padlock of four rollers is ſold at our ironmongers. I bought one at Downer's in Fleet Street, for ſixteen pence.

It would alfo be eafy to make it into a true lock of combination; by making each of the circular pieces to confift of two equal pieces, locking into each other by contrate teeth, and fcrewed together by a nut. And if one of thefe pieces were permanently fixed to the axis, and the other were to carry the edge-teeth, and notch, as mentioned in the laft paragraph, the lock would not only be fecure from opening by the method I ufed; but it would alfo be in the choice of the poffeffor to vary the fecret of the pofitions by unfcrewing the nut, and changing the application of the contrate teeth, after which he would fecure them together as before.

And alfo extended in its combinations.

XI.

Analyfis of a Pulmonary Calculus, by PHILIP CRAMPTON, M. D. *Member of the Royal College of Surgeons in Ireland. Communicated by the* HONOURABLE GEORGE KNOX. *F. R. S. M. R. I. A. &c* *.

SOME apology feems neceffary for offering the analyfis of a fingle pulmonary concretion, but the difficulty of procuring thofe fubftances, and the importance of every fact, however infulated, which relates to the changes that take place in the bodies of living animals muft plead my excufe.

Difficulty of obtaining pulmonary concretions.

I have no where been able to find a detailed analyfis of pulmonary calculi; Mr. Fourcroy mentions them in a general way, as being fimilar to lachrymal, pineal and falivary concretions, all of which he fays are compofed, as might be expected *a-priori*, of phofphate of lime (either from a fuperabundance of that fubftance in the fluids of furfaces, or in the blood)

They have not been examined.

But as the calculus in queftion neither agrees with any of thofe in its external characters, nor in its chemical compofition, without in the leaft calling in queftion the accuracy of Mr. Fourcroy, I fhall briefly ftate the refult of my own examination of it.

The calculus which is the fubject of this paper, was taken from the lungs of a deceafed foldier, in the year 1796, and

Defcription and hiftory of a pulmonary calculus.

* Read before the Royal Society in Auguft 1802.

was depofited in the mufeum of the Royal College of Surgeons in Ireland, It was irregularly fpheroidal and meafured about $6\frac{1}{2}$ inches in circumference. It appeared to be formed of fhort thick branches, proceeding from a folid center; its fracture was laminated, of a pure white colour, fmall communicating canals appeared to interfect its fubftance in every direction *. The faw cuts it with difficulty; 10 grains afforded an analyfis.

It was carbonate of lime.

grs.	
3.7	carbonic acid.
4.5	lime.
1.8	albumen and water.
10.0.	

The analyfis.

I fhall not trefpafs on your time by detailing the procefs of fo fimple an analyfis, fuffice it to fay, that it diffolved with effervefcence in the fulphuric, nitric, muriatic, and acetic acids, leaving fome floculi of coagulated animal matter (probably albumen) floating in the liquor of folution, the gas which was extricated precipitated lime from lime water, and the quantity of carbonic acid was afcertained, by adding a known quantity of muriatic acid to a certain quantity of the calculus, and taking the lofs after effervefcence for the weight of the carbonic acid which was extricated. A portion of the calculus expofed to a low heat affumed a blackifh hue, but when fubjected to a white heat for 20 minutes, it became of a brilliant white colour. The refidium was found to confift of pure lime. Morgani Epift. 17 to 19 upon the fubject of pulmonary calculi, fays, that he has feen many of thefe fubftances coughed up, that they were light and porous like pumice ftone, and varied from the fize of a millet-feed, to that of a pea; but he once faw a calculus which had been taken from the lungs of a perfon who had died of Thifis Pulmonalis, which weighed 20 grains, was *"hard and heavy like marble."* Dodenius faw a pulmonary calculus taken after death from the lungs of a nobleman, which affected the fhape of the bronchiæ, having fhort thick branches proceeding from it, and canals which interfected it in every direction. Morton faw fmooth cretaceous ftones taken from the lungs, Morgani Epift. XV. 23.

Other calculi refered to.

* Apparently formed by the depofition of calculous matter upon the bronchial veffels.

It would appear thence that there are at leaſt two kinds of pulmonary concretions: thoſe which Morgani obſerved to be thrown up by coughing, anſwer the deſcription of the calculi, examined by M. Fourcroy, and thoſe it has been ſtated were compoſed of phoſphat of lime, while the concretions which were hard and heavy like marble, it is probable agree in their chemical compoſition, as well as in their external characters with the calculus, which is the ſubject of this paper; this it has been proved was compoſed of carbonate of lime. A queſtion then naturally ariſes, as to the formation of carbonate of lime in the lungs. Was it depoſited as ſuch from the blood or formed in the Bronchiæ by the chemical combination of its principles? We have certainly not hitherto been able to detect carbonate of lime in the blood, and we have no reaſon to ſuppoſe that the phoſphat of lime which exiſts in it, can be decompoſed by the carbonic acid which is formed in the lungs, to avoid theſe alternatives, the chemical phyſiologiſt might be inclined to look to an external cauſe for the formation of this extraordinary ſubſtance, and with this view, he would enquire into the previous occupation of the perſon from whoſe lungs it had been taken; but he would be checked in his enquiries by the recollection that the ſame chemical affinities which can form carbonate of lime in the teeth and bones, may by an accdental coincidence depoſit it in the lungs.

Probability that there may be two kinds; viz. the carbonate and the phoſphate of lime.

XII.

Of the State of Vapour ſubſiſting in the Atmoſphere. By RICHARD KIRWAN, ESQ. LLD. F.R.S. *and* P. R. I. A *.

VAPOUR or moiſture in the atmoſphere may ſubſiſt in denſe air, or in air highly rarified: that it is found in the former is well known, and that it may ſubſiſt in the latter appears by the obſervations of Bouguer, for he ſaw clouds three or four hundred toiſes above Chimboracho, and conſequently at the height of twenty-two thouſand five hundred and twenty-eight Engliſh feet, or 4,3 miles over the level of the ſea; a height

The ſubſiſtence of vapour in denſe and in highly rarefied air.

* From his eſſay on the variations of the atmoſphere; In the Iriſh tranſactions, Vol. viii.

at

at which in the temperature of 32° a barometer would ſtand at 12,7 inches. At ſuch heights, and at much inferior, ſince evaporation proceeds much more quickly, it is not to be ſuppoſed that all the vapour ſo rapidly produced is diſſolved in the ambient air, but part riſes uncombined as it does under an exhauſted or half exhauſted receiver, and in this caſe Mr. De Luc's ſyſtem is admiſſible. This emiſſion of pure vapour ſeems to begin at heights at which the denſity of the air is 25 (that is at heights at which the barometer would ſtand at twenty-five inches, and thus I ſhall in future expreſs the various denſities of air,) at leaſt it is very conſiderable where the denſity is twenty, as already ſeen, p. 309. This leads me to treat of the properties and ſtate of pure inviſible vapour, namely, its ſpecific heat, elaſticity, and ſpecific gravity.

Latent Heat of ſteam.

The immortal Doctor Black, the father of all diſcoveries of this kind, informed me that the vapour of water, boiling at 212°, that is at 180° above the freezing point, and poſſeſſing the ſame ſenſible heat as the water, contains nine hundred and forty times more latent heat than an equal weight of water does heated to 212°, or 5,222 times more latent heat than it does of ſenſible heat, counting from the freezing point, for 180 × 5,222 = 940 nearly. In this caſe the preſſure or denſity of the atmoſphere is thirty, the barometer ſtanding at the height of thirty inches; and with Doctor Black's account the experiments of Mr. Schmidt of Gieſſen very nearly agree, for according to him the latent heat of the vapour of water, barometer 29,84 inches, and the heat 212°, is 5,33 times greater than its ſenſible heat above the freezing point, now 180 × 5,33 = 959,4 *. The difference or exceſs in his experiment proceeds from the preſſure of the atmoſphere being ſomewhat lower, as Mr. Watt's experiments prove.

— is leſs the greater its temperature.

Mr. Watt diſcovered that the latent heat of ſteam diminiſhed in proportion as its ſenſible heat increaſed, Phil. Tranſ. 1784, p. 335. Now the ſenſible heat of ſteam exceeds 180° above the freezing point when the barometer ſtands above thirty inches, and is leſs than 180° when the barometer ſtands lower than thirty inches, as Mr. De Luc firſt diſcovered, and may be ſeen in Sir George Shuckburgh's, and Mr. De Luc's tables, Phil. Tranſ. 1779. p. 375. From theſe I have deduced the following table:

* 4 Gren's Phys. Journal, p. 315.

Heat

Heat of boiling water.	
Bar.	Heat.
30	212°,
29	210,28
28	208,52
27	206,73
26	204,91
25	203,06
24	201,18
23	199,27
22	197,33
21	195,36
20	193,36
19	191,06
18	188,46
17	185,56
16	184,36
15	180,86
14	176,70

The accuracy of this table even in the lower part of the fcale is fufficiently apparent by the refult of the experiments of Sauffure on ebullition on Mount Blanc; for on that enormous mountain, the barometer ftanding at 16 French inches or 17,05 Englifh, he found water to boil at the heat of 68°,993 of Reaumur, a degree which on Geneva thermometers is equal to 185°,56 Englifh. Hence we fee that diftillation may be more advantageoufly effected on mountains than on plains, and at low barometrical heights than at the greater, yet within certain limits; for at heights that furpafs eight or ten thoufand feet, the fuel, by reafon of the rarity of the air, is more flowly confumed. Hence alfo from the knowledge of the degree of the heat of ebullition to two or more decimal places, the ftate of the barometer above or below 212° may infer to one or more decimal places.—The reafon of this rapid diminution of the heat of ebullition below 25 inches is evidently the diminution of refiftance, from the diminifhed weight of the atmofphere, which then is very fenfible; but as the cold continually produced by evaporation is then alfo very confiderable, the *time* neceffary to procure ebullition is longer as Sauffure remarked on Mount Blanc. vol. vii. in 8vo. § 2011, p. 328. He found the heat of ebullition barometer 16 to be 68°,993 degrees, or in Englifh meafures barometer 17,05. 185°,5 of Fahr. (counting one of Reaumur at Geneva = 2,225 of Fahr.)

Table of the temperatures of boiling water at different ftations of the barometer, from 30 inches to 14 inches.

Hence fince, according to Mr. Watt, the fenfible heats of the vapours of boiling water at different barometrical heights are as the barometrical heights reciprocally, and the fpecific heats of the vapours of water boiling are as the fenfible heats reciprocally, it being known, that the fpecific heat of the vapour of water heated to 180 degrees above the freezing point is 940. The fpecific or latent heat of the vapour of boiling water, whofe fenfible heat is known, (and it may be known by the barometrical height as fhewn in the above table and the notes) may alfo be difcovered.

Hence the fpecific heat of fteam at different temperatures.

Thus the fenfible heat of the vapour of boiling water barometer 30 being 180° above the freezing point (212° − 32° = 180°) and the fpecific or latent heat of vapour, whofe fenfible

Inftance.

ſible heat is 208°,56 (that is 176,56 above 32°) as it is when the barometer ſtands at 28 inches, is 958 for ∺ 176,56. 180 : : 940.958 *.

Uncombined vapour conſidered not to exiſt.

As pure inviſible vapour does not in my opinion (of which I have already ſtated the grounds) exiſt in the atmoſphere when its denſity is higher than 25, as it is in moſt of the inhabited parts of the globe, but is always in this caſe united to air, an enquiry into its latent heat at different temperatures below ebullition were ſuperfluous. But as it does exiſt in air whoſe denſity is 25 or leſs, ſince it is found in air whoſe denſity is 12,5, it becomes neceſſary to examine its latent heat in ſuch caſes, in all temperatures inferior to that of ebullition. Now, by analogy, I apprehend, this latent heat in all inferior temperatures may thus be determined :

Determination of the latent heat of pure vapour.

As the *ſenſible* heat of ebullition, when the barometer is at 25 or below 25 is to the *latent* heat of the vapour at ebullition, ſo is the *ſenſible* heat of water heated to any inferior degree above 32° to the latent heat of its vapour, multiplied by 5,222. Thus the ſenſible heat of water in ebullition barometer 25 being 171°,4 (= 203°,4−32°) its ſpecific heat is 987 $\left(=\frac{169206}{171,4}\right)$ the latent heat of the vapour of water at 22° above congelation (that is 52 on Fahr. ſcale) is 657 for ∺ 171°,4. 987 : : 22°. 126 × 5,22 = 657. The latent heat of vapour in ſuch caſes cannot be determined by experiment on account of the admixture of atmoſpheric air, we muſt therefore reſort to analogy, which in this caſe is perfect.

The latent heat of pure vapour at greater heights is more conſiderable : thus at heights, at which the barometer ſtands at 20 inches, the latent heat of vapour whoſe temperature is 22° above 32°, as in the laſt caſe, is 730 ; for the heat of ebullition is 194°,8, per table, = 162°,8 above 32°; and the latent heat of the vapour at ebullition is 1039. Now ∺ 162,8. 1039 : : 2°. 140 and 140 × 5,22 = 730.

As air is cooled by the reception of moiſture diſſolved in it, we muſt infer that its capacity for containing heat is increaſed, and hence moiſt air is more difficultly heated or cooled than dry air of the ſame temperature. (For the cold proceeds from the abſorption and not from the expulſion of caloric.)

* Hence 169206, being the product of 180° × 940, is the common dividend of all ſenſible heats *below* 180°. when the latent heat of the vapour is ſought at barometrical heights below 30 inches.

 The

The elasticity or expansive force of pure vapour has been examined at every fifth degree of Reaumur above 0 to 110°. by Mr. Betancourt, and may be seen in an excellent work of Prony's, his *Architecture Hydraulique*, he has by a most ingenious calculation interpolated the expansions answering to the intermediate degrees. But Mr. Schmidt seems to have determined this expansive force still more exactly than Betancourt. Hence I here insert his table, adding Farenheit's for Reaumur's degrees, and distinguishing the expansions interpolated by calculation from those actually observed by *I*. The forces are measured by the elevation of a mercurial column in inches and hundreds of a French inch *.

Elasticity of vapour at temperatures above freezing.

Reaumur.	Farenheit.	Expansive Force.	Reaumur.	Farenheit.	Expansive Force.
1	34°,25	,01 *I*	21	79°,25	1,01 *I*
2	36,5	,03 *I*	22	81,5	1,01
3	38,75	,05 *I*	23	83,75	1,19 *I*
4	41,	,07 *I*	24	86,	1,29 *I*
5	43,25	,11	25	88,25	1,30
6	45,5	,15	26	90,5	1,38 *I*
7	47,75	,16 *I*	27	92,75	1,42
8	50,	,20 *I*	28	95,	1,60 *I*
9	52,25	,25 *I*	29	97,25	1,80 *I*
10	54,5	,28	30	99,5	1,93
11	56,75	,34 *I*	31	101,75	1,02 *I*
12	59,	,38	32	104,	1,12 *I*
13	61,25	,44	33	106,25	2,23
14	63,5	,50 *I*	34	108,5	2,40 *I*
15	65,75	,55	35	110,75	2,68
16	68,	,61	36	113,	2,80 *I*
17	70,25	,69 *I*	37	115,25	3,20
18	72,5	,76	38	117,5	
19	74,75	,84 *I*	39	119,75	3,40
20	77,	,90	40	122,	3,64
			80	212,	28,

* The Paris cubic inch = 1,21 English. Now the English cubic inch of mercury when its specific gravity is 13,6 weights 3443,2 English grains, therefore the Paris inch weighs 4186 English grains, and $\frac{1}{10}$ of this inch = 418,6 grs: and $\frac{1}{100}$ of this inch 41,86 grains.

Note.—1ſt. Moſt of the interpolations from the 88th degree to the 122nd I have myſelf inſerted as thoſe calculated by Schmidt erred too widely by his own account. 4 Gren. Phyſ. Journ. 273.

2d. Mr. Pictet has alſo made a ſet of curious experiments on the elaſticity of pure vapour in low temperature. *Eſſais de Phyſique*, p. 157. He found that a grain of warm water in *vacuo* evaporates in forty minutes in the temperature of 38° Fahr. under a receiver containing 1452 Engliſh cubic inches,* but that it did not diffuſe itſelf equally in leſs than ſix hours, and then raiſed the hygrometer from 17° to 60° that is 43°, and during this whole time the cold under the receiver was conſtantly decreaſing, though ſlowly, which decreaſe undoubtedly contributed to the diffuſion of the vapour.

Mr. Schmidt has alſo made a ſeries of experiments upon the dilatibility of air, made as *dry* as poſſible by expoſure to hot tartarin, an object of great importance, that had never before been examined. This table I here inſert, converting Reaumur's degrees into thoſe of Fahr. and adding from his formula the degrees he omitted.

Expanſions of dry Air.

Table of expanſions of dry air.

Reaumur.	Fahr.	Expanſion of one Inch at 32	Reaumur.	Fahr.	Expanſions of one Inch at 32°.
1	34°,25	,004467		79°,25	,0938175
	36,5	,008935		81,5	,0982850
	38,75	,0134025		83,75	,1027525
4	41,	,0178700	24	86,	,1072200
	43,25	,0223375		88,25	,11116875
	45,5	,0268050		90,5	,1116155
	47,75	,0312726		92,75	,1206225
8	50,	,035740	28	95,	,1250909
	52,25	,0402075		97,25	,1295557
	54,5	,0446750		99,5	,1340250
	56,75	,0491425		101,75	,1384925
12	59,	,0536100	32	104,	,1429600
	61,25	,0580775		106,25	,1474275
	63,5	,0625450		108,5	,1518950
	65,75	,0670125		110,75	,1563625
16	68,	,0714800	36	113,	,1608300
	70,25	,0759475		115,25	,1652975
	72,5	,0804150		117,5	,1697650
	74,75	,0848825		119,75	,1742825
20	77,	,0893500	40	122,	,1787000
				212,	,3574000

* Ibid. page 91.

Note.

Note.—1. Hence we ſee that 1000 inches or meaſures of dry air at 32° would become 1004,4675 at 34,25 Fahr. and at 50°, would become 1017,87. Hence 1000 meaſures of *dry* air gain 1,985555, &c. by each degree of Fahr. above 32° (or more compendiouſly 1,9856 which is true to two decimal places) or nearly two.

2. We ſee the ſource of the diſcordant reſults of D'Amontons, De Luc, Lambert, Schuckburg, Roy, Berthollet, and Monge, &c. for they all operated upon air more impregnated with various degrees of moiſture; beſides taking the boiling point at different barometrical heights; in the preſent experiments it was taken at 29,841 Engliſh inches.

3. It appears that the expanſions are as the differences of heat above 32° as D'Amontons, Lambert, and Schuckburg alſo noticed, though their experiments, not being made on perfectly dry air, could not be very exact.

The dilatation of the moiſture contained in air has been ſeparately examined by Mr. Schmidt, and he has ſhewn how from it the volume of air ſaturated with moiſture, ſaturated I ſay at every degree of Reaumur, may be diſcovered; the reſult of his experiments appear in the following table of the volume which 1000 meaſures at 32° of air would acquire if *ſaturated with moiſture* at each degree of Reaumur above 32° expreſſed on Farenheit's ſcale *.

Dilatations of air ſaturated with moiſture.

Reaumur.	Fahr.	Expanſive Force.	Reaumur.	Fahr.	Expanſive Force.
1°	34°,25	1010,56		70,25	1122,68
	36,5	1010,78		72,5	1132,25
	38,75	1016,45		74,75	1142,53
	41,	1022,21	20	77,	1152,83
5	43,25	1028,58		79,25	1164,02
	45,5	1034,97		81,5	1175,23
	47,75	1040,41		83,75	1186,52
8	50,	1048,52	24	86,	1198,59
	52,25	1056,26	25	88,25	1211,44
10	54,5	1064,72		90,5	1223,65
	56,75	1071,28		92,75	1279,62
12	59,	1078,52	28	95,	1377,09
	61,25	1087,11		97,25	1494,02
	63,5	1095,76	30	99,5	1610,02
15	65,75	1104,46		101,75	1725,49
16	68,	1113,21	32	104,	1849,96
			33	106,25	1983,42

* To prevent miſtakes, it muſt be noted that this table is not

meant

Remarks. Air ſaturated with moiſture expands more than dry air.

Note.—1. Hence we ſee that air ſaturated with moiſture at high heats is much more expanded than dry air of the ſame temperature, as De Luc and General Roy have alſo obſerved. but in temperatures below 36°,5 dry air is more dilatable which probably induced Sauſſure to conclude it was ſo at higher temperatures. At 54,5 the difference is very perceptible for 1000 parts *dry* air at 32° are expanded at 54°,5 that is by 22°,5 above the freezing point to 1044,67, whereas 1000 parts of air ſaturated with moiſture, are extended to 1064,72 and in higher heats, the differences of expanſion are incomparably greater.

Moiſt air more ſuffocating than dry.

2. Hence it is plain why moiſt air, ſuch as that of the Weſt Indies is much more ſuffocating than dry air of the ſame temperature. For 1000 cubic inches of air ſaturated with moiſture at 86° of Fahr. contain nearly 76 inches of moiſture which is uſeleſs to reſpiration.

Gen. Roy's experiments.

3. Theſe experiments agree with thoſe of general Roy, in which ſteam was introduced at hazard, for the general found that from 32° to 52° each degree gave at a mean 2,588, and conſequently theſe 20° would expand 1000 inches to 1051,76, and by Schmidt's experiments much more accurately made, we have 1050,33.

Irregularity of the expanſions of moiſt air,

4. Schmidt alſo obſerved a peculiarity in the expanſion of moiſt air, previouſly noticed by Roy, for Schmidt found that the expanſibility of air, ſaturated with moiſture, was ſmaller than the expanſibility of pure vapour, until the 167th degree of Fahr. but in higher degrees they conſtantly approached nearer to each other. And the general obſerved that the mean rate of expanſion, which from 152° to 172° of Fahr. was 12 for each degree, did from the 172° to the 192° increaſe to 17,88 for each degree, and increaſed ſtill more after the 192d to the boiling point. The ſluggiſhneſs of expanſion of air, ſaturated with moiſture at about 32°, was alſo noticed by the general, and he hence concludes the mean rate of expanſion from 0 to 32° of Fahr. to be 2,27 for each degree, which is ſmaller than that of drier air.

meant to expreſs the dilatations of air ſaturated at any particular degree of heat it would acquire at other ſuperior degrees, but only the bulk that 1000 parts dry air at 32° would acquire by ſaturation at each higher degree.

Theſe

Theſe variations of the rates of expanſibility of moiſt air, ſaturated at different temperatures, Schmidt very juſtly attributes to the variation of the degrees of affinity or adherence of air and vapour to each other at different temperatures. At 32° Fahr. it is very ſtrong, and alſo below that degree; and hence the ſtrong ſolvent power of air, colder than the water it acts upon, remarked by Richman; but if both are equally cold very little moiſture will be taken up by the air, as already mentioned; and hence I have ſaid that air diſſolves vapour when this is in a naſcent ſtate. But in heats above 167° or 170° air and vapour are diſpoſed to ſeparate.

aſcribed to difference of affinity at different temperatures.

5. Hence we may deduce the impoſſibility of diſcovering a coefficient univerſally applicable to expreſs the rate of expanſion of air in every ſtate of moiſture, (as Tremley has well noticed. See 2 Sauſſure Voy. aux Alpes 4to). This muſt vary with the mean ſtate of hygrometers above and below the heights to be meaſured; and experiments of this kind have not yet been made. De Luc's coefficient anſwers tolerably well for very dry air, that is whoſe ſaturability is greateſt, Sir George Schuckburg's for air much moiſter, and general Roy's for air ſtill more moiſt, that is whoſe ſaturability is ſmalleſt. Hence each ſucceeds in certain caſes, and fails in others. The dilatation or contraction, which air ſaturated with moiſture at any one given degree of temperature receives without the addition of any more moiſture, at any higher or lower degree of temperature, has not as yet been diſcovered; for Schmidt, who alone has attempted it, is juſtly diffident of the correctneſs of the table he has given of it, and in fact it is not grounded on the indication of any known hygrometer, and improperly ſuppoſes the L. degree to indicate the mean betwixt the loweſt and ſaturation. Whereas the LXVth degree on Sauſſure's indicates that mean; and XCVIII. and not C. indicates ſaturation.

Hence there can be no determinate coefficient to expreſs that rate.

According to Mr. Watt (as ſtated by De Luc, 3 Meteorolog: p. 145) the ſpecific gravity of pure vapour is to that of air as 4 to 9. I ſuppoſe he compares it with air at the uſual denſity of 30 or 29, and at ſome particular temperature which is not mentioned; for at high temperatures the difference muſt be much greater, as appears by the foregoing tables.

Specific gravities of vapour and of air.

(To be concluded in our next.)

XIII.

Notices concerning ſome Philoſophical Apparatus.—Electrical Battery of Talc.—Complex Horſeſhoe Magnet.—Mr. DAVY'S *Apparatus, &c.*

Battery of talc.

SEVERAL years ago (in 1788), when I was buſied in electrical reſearches, I was induced to conſtruct a battery of talc, in conſequence of the very great capacity of that ſubſtance which I found (conformably to the determination of Mr. Cavendiſh with glaſs), to be inverſely as its thickneſs. As our galvanic reſearches now ſeem to give additional intereſt to apparatus of great capacity and low intenſity, I have thought it might be acceptable to give a ſketch of it.

Eſtimate of the electrical capacity of talc. It is as the thickneſs inverſely; or eight times that of glaſs.

The full charge of two ſquare inches of talc gives a ſhock ſufficiently ſevere to be felt above the elbows, giving a ſpark of above one tenth of an inch long, and requiring the friction of more than one foot of glaſs to charge it. I compared this talc with a large jar of 351 ſquare inches, and 0.082 inch thick, and the talc was 0.01125 inch thick. The communication from plus to minus was made by one ſingle wire, and Lane's electrometer was ſet ſo that the talc exploded regularly at each turn of a ſeven inch cylinder. The jar was then placed in the circuit, and exploded at twenty-one turns. Their abſolute capacities were therefore as 1 to 21. But the ſurfaces are as 1 to 175, whence the capacities of equal ſurfaces were as 1 to $\frac{175}{21}$, or about 1 to 8. And the thickneſſes are inverſely 11 and 82, whence it appears that the actual capacities proved rather in favour of the talc, if taken from the inverted ratio of the thickneſs. This, however, may be only apparent, becauſe the talc was more rapidly charged, and had an inſulated rim of only 0.4 inch, whereas the glaſs had a bare rim of 4.0 inches. In addition to which it may be obſerved, that the uncertainty of the reſult is not quoted, than the unavoidable errors from the admeaſurements of the thickneſſes and the unſteadineſs of excitation.

Drawing and deſcription of a talc battery two inches ſquare, and equivalent to ſeven ſquare feet of glaſs.

Plate XI. Fig. 1. repreſents a battery compoſed of 12 pieces of talc, each meaſuring 2.6 inches in the ſide, and coated on a ſurface of two ſquare inches. The thickneſs of each piece was 0.0025 inch, and conſequently its capacity may be taken at about 20 times that of thin window glaſs of one-twentieth

2 of

of an inch thick. The whole surface was 48 square inches, which multiplied by 20, answers to 960 square inches of glass in capacity, or almost seven square feet. It is necessary that a card should be placed between each plate of talc and its neighbour, and hence by an easy computation, it is found that the talcs and cards (of $\frac{1}{50}$ of an inch thick), requisite to answer to 100 feet of glass would be about $3\frac{1}{2}$ inches thick.

This instrument for the purposes of low intensities would be incomparably cheaper and more manageable than glass. Indeed a battery answering to twenty thousand square feet would be very portable, and might be put into a box of one foot square and two feet deep. But it must be remembered that the intensity cannot be greater than to give an explosive spark of one thirteenth of an inch, or less, and consequently that this apparatus is limited to experiments requiring no greater intensity. Size of a battery equivalent to 20,000 square feet.

The construction is that described by Beccaria, in his artificial electricity. Fig. 2. represents the arrangement of the plates of talc with square coatings of tinfoil between them. From each coating alternately succeeds a tail, so that all the positive sides unite above, and all the negative below. In Fig. 1. the wire *c* communicates with all the coatings of one kind, and the wire *a* in like manner unites all the coatings from the opposite sides; and at *b* is an insulated screw wire forming the electrometer of Lane. The pillar supporting the screw *e* may either be of glass or other insulating matter, or else a piece of amber, *d* may be placed between the battery and *e*. In my apparatus, the talcs were confined between two squares of glass, and on the outside of these were two smaller pieces of wood as in the figure. Beccaria's battery.

I did not make many experiments with this battery, because the spontaneous explosions of one plate usually broke all the rest, and it was my intention to have them somewhat thicker, and to have interposed cards between plate and plate. But other pursuits have hitherto prevented my resuming this object.

Complex Horseshoe Magnet.

Fig. 3. in the same Plate XI. was given me by the late George Adams of Fleet Street. It was wrapped in a paper on which the workman who made it had written the following words. Complex horseshoe magnet.

" The

"The outſide bit taken off, but drops when the inſide one ſticks, but drops when the outſide one is put on. Apply the magnet as directed, the ſtrokes to be north to north ſtrait acroſs the inſide, very difficult to gain, but may be otherwiſe varied at pleaſure."

has four poles and four neutral points, which are changed in their power by the conductors.

When I firſt had it, the middle piece would not drop off when the outer was put on, but on the contrary, the outer was diſengaged by application of the inner piece, and this is the caſe at preſent. I could not obtain any explanation of the obſcure writing I have copied. The ſketch ſhows where the poles were at firſt and ſtill are placed, being four in number, ſo that it ſeems to be two horſeſhoe magnets joined by a ſhort bar nearly ſtrait. It barely held its own weight by the outer piece, and much leſs by the inner; but now, whether by the courſe of time in which it has been careleſsly thrown in a drawer among other metallic matters for ſixteen years, or whether from any other cauſe, its attraction is ſtrongeſt at the inner extremities though not ſufficient to bear its weight.

Galvanic Apparatus.

Davy's apparatus for galvanizing the gaſes.

Plate X. Fig. 2 and 3. repreſents the apparatus of Mr. Davy for taking the galvanic ſpark in the gaſes*.

Fig. 1. repreſents the apparatus for taking the ſpark. A is a tube graduated to grain meaſures. C is a platina wire hermetically ſealed into the tube and having a piece of charcoal attached to its top. B is a moveable platina wire, having charcoal at the top; the effect is produced by making the contact between the piece of charcoal. In caſes where the fluids are very imperfect conductors, the wires may be uſed without the charcoal.

Fig. 2. repreſents the apparatus for taking the ſpark in gaſes, it is uſed over mercury. A and B are the communicating platina wires to which the charcoal is faſtened, and C is the graduated tube in which the gas is acted upon.

* Royal Inſtitution, I. 214.

XIV. *Experiment*

XIV.

Experiment to determine the relative Quantities of Light afforded by Candles of different Dimenſions, in a Letter from a Correſpondent.

To Mr. NICHOLSON.

SIR,

THOUGH your ingenious correſpondent Mr. Ezekiel Walker, aſſerts with conſiderable deciſion, that the light afforded by candles, is proportioned to the quantity of material conſumed, yet as he has not given the detail of his experiments, but ſeems in ſome meaſure to have deduced this reſult by argument, from the ſuppoſed nature of the ſubject; and as the quantity of light does undoubtedly follow a very different ratio in candles which require ſnuffing, accordingly as that operation is either performed or neglected. I think it very deſirable, that a courſe of experiments ſhould be inſtituted for the purpoſe of affording the reſults you have yourſelf enumerated in your annotation upon one of that gentleman's papers. In the mean time, I preſent you with a ſingle experiment upon his two wax candles, which were taken by me in preference, becauſe they require no ſnuffing.

Prefatory remarks on Mr. E. Walker's experiments on light, &c.

One of theſe candles was a taper of ſixteen to the pound, and meaſured in length twelve inches; the other was a thicker candle of ſix to the pound, and nine inches long. It is not neceſſary to ſtate the diameters, becauſe they are not required in the preſent eſtimate, and might if neceſſary, be deduced with greater preciſion from the weight and length, than from actual admeaſurement; and with regard to the wicks, I have only to remark, that they were well adapted to the candles—though it was impoſſible for me to aſcertain their weight or fineneſs in the manufactured ſtate.

Experiment with a thin and a thick wax candle.

Theſe two candles were lighted at the ſame time, and when they were in the ſtate of full and perfect combuſtion, their lengths were reſpectively meaſured and noted down. The intenſities of their lights, were compared by the method of ſhadows. The diſtance between the two candles was twenty-four inches; and when the ſhadows were of equal intenſity, the diſtance of the ſcreen from the neareſt candle was ſeventy-

two

Experiment with a thin and a thick wax candle.

two inches. The ſhadow into which the large candle threw its light, was evidently ruddy or browniſh, while the other ſhadow illuminated by the ſmall flame, ſeemed to have a bluiſh tinge. The flames themſelves appeared to differ in their aſpect, in the ſame manner as their ſhadows: now the lights emitted being inverſely as the ſquare of the diſtances, will be as 36 to 64, or as 9 to 16; and when both were extinguiſhed, it was found that three inches of the large candle had been burned, and 5.8 inches of the ſmaller: but of the larger candle, fifty-four inches in length make one pound, and of the ſmaller 192 inches make the pound; and if we divide 54 inches by three, the quotient will be 18, which ſhews, that the whole pound of large candles would have required eighteen times our period for its conſumption: in like manner, by dividing 192 by 5.8, we have a quotient of 33.1, or the time of burning a pound of the ſmall candles. And laſtly, if we multiply the quantity of light afforded by each candle, by the time required to conſume a pound of each, we ſhall have the proportionate quantities of light afforded by equal expenditures of the combuſtible, that is to ſay, 18 multiplied by 16, produce 288, or the light afforded by the larger candle, and 33.1 multiplied by 9, gives 297.9 for the ſmaller: which is one part in 29 in favour of the ſmall candle.

I conſider this difference as a greater quantity than the errors of experiment, though upon the whole, it ſeems to confirm Mr. Walker's doctrine.

SCIENTIFIC NEWS.

On the Nutriment to be obtained from Bones.

Reſults as detailed by Prouſt.

IN the firſt volume of the octavo ſeries of this Journal, page 100, notice is taken of the experiments on this ſubject, detailed by profeſſor Prouſt of Madrid, in his inquiry into the means of improving the ſubſiſtence of the ſoldier. He ſtates the quantity of jelly obtained from bones to be very variable, according to the nature of thoſe employed, viz. from nine pints to thirteen quarts, from five pounds of bones; thoſe from the rump and ſpine yielding the moſt and beſt; and thoſe of the legs and thighs the leaſt and moſt unpalatable.

Sinc

Since his work a memoir on the gelatine of bones has been publiſhed at Paris by Cadet de Vaux, extracts from which have been circulated through France by order of the miniſter of the interior, in which this method of making a wholeſome and pleaſant food is ſtrongly recommended. His experiments are ſaid to have afforded reſults nearly ſimilar to thoſe of Prouſt, but on being repeated by Dr. Young ſeem to have completely failed. His experiment which is inſerted in the fifteenth number of the Journals of the Royal Inſtitution, is as follows: By Cadet de Vaux.

Three quarters of a pound of bones were taken from a piece of beef which had been roaſted; they were principally the vertebral and ſpinal proceſſes; they were reduced to a ſmooth paſte, by pounding them for twenty minutes, with a little water, in a large mortar. They were kept on the fire in an earthern pipkin, ſimmering or ſlowly boiling, for forty hours, with the addition of proper vegetables, ſo as to make a quart of broth. The broth ſhewed no tendency to jelly when cold; it taſted only of the vegetables, and had neither the appearance, nor the flavour of a highly nutricious ſubſtance: there was nothing diſagreeable in it except its inſipidity, but a few ounces of meat would probably have made a better ſoup. Dr. Young's experiment.

Method of preparing Muriatic Ether with ſimple Acid; by M. Bossa, *Apothecary at Hameln*.* Preparation of muriatic ether by *Baſſe.*

MELT marine ſalt in a crucible and keep it in fuſion an hour, or till the whole of the water of cryſtalization is diſſipated. Put twenty ounces of this ſalt into a tubulated retort, adapt a curved tube to it, and plunge the tube to the bottom of a bottle with two necks, into which have been poured ten ounces of alcohol, marking 100 on Richter's alcoholimeter. (This alcohol muſt be prepared by mixing three parts of highly rectified ſpirit of wine with one part of potaſh, melted and pulverized whilſt hot, and diſtilling it in a retort till it be diminiſhed one half.) When the whole is well luted, pour into the retort, in quantities of half a penny weight at a time,

* From Scherer's Journal de Chemie.

ten

ten ounces of highly concentrated fulphuric acid. After each introduction of acid, clofe the tubulure carefully, and put in no more frefh acid till the falt has quite ceafed bubbling. The cork of the other neck of the bottle muft be taken out from time to time, to fuffer the air condenfed above the alcohol to efcape.

After all the acid is introduced, place the retort on a fand-bath, and heat it gradually till all the muriatic acid be expelled. During this part of the operation, care muft be taken frequently to cool the bottle containing the alcohol, by wrapping a wet cloth round it.

The alcohol, thus charged with acid, is then put into a retort and diftilled to one half. Shake the diftilled liquor with a fufficient quantity of alcaline ley to carry off the acid, decant the fupernatant portion, which is ether, and keep it in bottles well corked. From the above mentioned quantities, two ounces and a half of ether are ufually obtained.

M. Boffa afferts, that by operating with thermoxy dated muriatic acid, light alcoholic ether can never be obtained, but only a heavy oleaginous ether, which, inftead of floating on water, falls to the bottom of that liquid.

On the Effects produced by the Decortication of Trees.

Decortication of trees.

SEVERAL papers have lately appeared in the American Philofophical Tranfactions, and other fcientific works, recommending the depriving trees of their bark as a means of exterminating noxious infects which find fhelter in the crevices of the bark, and between it and the body of the tree. From the effects produced on two apple-trees, as related in the Monthly Magazine for laft month, though the fource from which their information is derived does not appear, it would feem that this practice is deftructive of the tree itfelf. The relator afferts, that, one of the trees died in May following the operation, and the other before the neighbouring trees had fhed their leaves. He alfo ftates that the denuded trees were affected by feveral long fiffures, which he attributes to the alterations of heat and cold, and confiders as the immediate caufe of their death. He concludes by a query, whether

whether ſheltering the trunks by matting, or other effectual means might cauſe the ſucceſs of the ſcheme, and recommends the experiment.

The ſociety of agriculture, commerce, ſciences and arts at Chalons, in the department of the Marne, have repropoſed the following prize queſtion, to which no ſatisfactory anſwer was received laſt year.

"What deſcription of uſeful plants of any ſpecies, can be made to grow on the moſt barren ſoils, ſuch as thoſe of the ancient Champagne (or department of the Marne) which preſents little or no vegetable earth, above a chalky or ſandy tufa?" Prize queſtion. Plants for barren ſoils.

In conſequence of the importance of the queſtion, the ſociety have increaſed the value of the gold medal, from 1000 to 1500 grammes of ſilver, or 300 livres; it is to be decreed at the public ſitting of the Society, on the 1ſt of Vendemiaire in the 12th year.

ACCOUNT OF NEW BOOKS.

Reſearches into the Properties of Spring Water with Medical Cautions (illuſtrated by Caſes) againſt the Uſe of Lead in the Conſtruction of Pumps, Water Pipes, Ciſterns, &c. By WILLIAM LAMBE, *M. D. late Fellow of St. John's College Cambridge, pp.* 204. London, 1803. Lambe on ſpring water.

THE object of this work is an inveſtigation of the qualities accidentally acquired by water, which render it in many inſtances injurious to health. Of theſe, lead is a principal cauſe, which in whatever form it is introduced into the human body, is equally deleterious and fatal. In treating of this poiſonous metal as a receptacle or conductor of water, he ſhows how it is acted on by it and air, ſo as to be capable of ſuſpenſion in the fluid, by which means it is received into the alimentary canals, and conveyed to every part of the ſyſtem. Hence ariſe diſeaſes, which from their cauſe being unſuſpected, and conſequently ſuffered to continue its effects, are unconquerable by medical ſkill. Many inſtances of this deſcription are given, ſome of which were removed by change of reſidence, and recurred on the return of the patient, nor were

were they effectually subdued, until the discovery of the cause, and the discontinuance of the use of the water which produced them.

Mr. Lambe has shewn a persevering attention to the subject he has undertaken to investigate, and has very clearly proved the noxious effects arising from the unguarded use of lead in domestic economy.

Sheldrake on ruptures.

Useful Hints to those who are afflicted with Ruptures, on the Nature, Cure, and Consequences of the Disease; and on the Empirical Practices of the present Day. By T. SHELDRAKE, *Truss-Maker to the Westminster-Hospital, pp.* 180. London, 1803.

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THE ARTS.

AUGUST, 1803.

ARTICLE I.

On the Production of Sulphate of Magnesia from the Ashes of Pit-Coal, with Remarks on the Efflorescence of the same Salt, observed by Dr. Bostock. In a Letter from the Rev. WILLIAM GREGOR.

To Mr. NICHOLSON.

SIR,

Efflorescence from ashes of pit coal, which afforded gypsum and sulphate of magnesia.

IN the number of your Philosophical Journal for December, 1802*, I read a letter from Dr. Bostock of Liverpool, in which he gives an account of an unexpected production of the sulphate of magnesia; which brought to my recollection a fact, which fell under my notice, about two years ago, and which may possibly tend to throw some light upon the phænomenon observed by your correspondent.—Having accidentally discovered a saline crust adhering to the wood-work of a frame on which I sift my coal-ashes, for economical and agricultural purposes, I scraped off a sufficient quantity of this substance with a view of ascertaining its nature. I dissolved it in distilled water, and separated all impurities from it, by the filter. This solution tried by the usual tests, gave indications of the presence of sulphuric acid, of lime, and of some other earth. I evaporated the solution, and some gypsum was separated; when this salt ceased to appear I poured off the fluid, which had assumed

* Vol. III. p. 288.

a browniſh hue. I gradually evaporated it ſtill further, and ſet it aſide in a cool place: It ſhot into a beautifully cryſtallized ſalt, which agreed in form and properties with the pureſt ſulphate of magneſia, except as to colour, which inclined towards a browniſh tint, which circumſtance probably aroſe from the extractive matter of the decayed wood, which this ſalt had penetrated. The coals, which had been ſifted in this place, were thoſe from Liverpool and Wales. The place was open to the air and rain.

Liverpool coal treated more carefully afford a purer reſult.

In order to narrow my field of conjecture as to the origin of the above-mentioned ſalts, and to exclude, as far as poſſible, the concurrence of unknown circumſtances, I collected a ſufficient quantity of aſhes from my own fire in my ſtudy, where I had burnt Liverpool coals only. On repeating the ſame proceſs, the reſult was the ſame: I obtained gypſum and a ſtill purer ſulphate of magneſia: The quantity of the latter exceeded that of the former. I examined alſo the aſhes produced by Welch coal, and I detected gypſum and the ſulphate of magneſia in them alſo. They contained, however, a much larger quantity of gypſum than of the latter ſalt.

Welch coal alſo.

The exact quantities of theſe ſalts, which might be obtained from a given weight of the aſhes of theſe different coals, I did not aſcertain.

Theſe aſhes were a good manure.

The aſhes obtained from the above-mentioned coals were from time to time ſpread, (or to uſe our common and provincial term) *ſkeaded* * over graſs, and with apparently good effects, notwithſtanding the ſulphate of magneſia, which, I was well aſſured that they contained.

Mr. Tennant has proved that magneſia and its carbonate are inimical to vegetation, but the ſulphate may be good.

Mr. Tennant's diſcovery of the unfriendly nature of magneſia to vegetation is to be ranked amongſt the few prominent and well eſtabliſhed facts in agriculture, which modern ſagacity has brought to light. Communications of facts and diſcoveries in this highly important ſcience are, indeed, abundantly ſupplied unto us by ingenious and experienced agriculturiſts, but the truth and the utility of the greater number of them muſt, from the nature of the ſubject, depend upon ſuch a variety of variable circumſtances, that the application of them to particular caſes can be only conſidered as a matter of experiment. But Mr. Tennant's obſervations, contained in his eſſay on the

* Etymologiſts may probably recognize in this word the Greek verb Σχέδαω.

5 different

different ſorts of lime uſed in agriculture, ſeem to ſtand, in a great meaſure, independent of local and contingent circumſtances, and may be ſafely recorded amongſt the certainties of the ſcience. But in this, as in other ſubjects, we muſt be careful, that our concluſions do not wander beyond the limits which our premiſes warrant. Magneſia and carbonate of magneſia may be inimical to vegetation: But it by no means follows from the admiſſion of this fact, that magneſia combined with ſulphuric acid poſſeſſes the ſame deſtructive properties.

I conjectured that the origin of theſe ſalts in coal aſhes was to be aſcribed to the decompoſition of ſchiſtus and pyrites, both which ſubſtances are frequently found amongſt pit coal. Origin of theſe ſulphates.

In order to form a judgment reſpecting the probability of my conjecture, I mixed ſome pounded pyrites with ſome ſteatite reduced to powder, in the proportion of about 2 : 1 and expoſed this mixture in a crucible for about an hour and a half to the heat of a briſk fire in a common grate. There remained at the bottom of the crucible a dark red powder, which was expoſed to the air for a conſiderable time before I examined it.—I threw it upon a filtre and poured diſtilled water upon it, which taſted exceedingly bitter, and on further examination, I found it to contain ſulphate of magneſia. Pruſſiate of potaſh did not betray the leaſt trace of iron held in ſolution. Whether the magneſian ſalt was in a ſtate of ſulphite at firſt I cannot tell. Pyrites and ſteatite ignited and then expoſed to the atmoſphere afforded ſulphate of magneſia.

In places where ſteatite and pyrites are plentiful, this proceſs for obtaining ſulphate of magneſia would be neither difficult nor expenſive.—In the caſe which Dr. Boſtock records, might there not have been ſome coal aſhes mixed with the clay uſed for making the brick, on which he diſcovered the ſaline efflореſcence? This artificial proceſs may perhaps be profitable.

As I have never ſeen the foregoing facts noticed by any perſon, I have been induced to tranſmit an account of them to you. If you think them worthy to be recorded, they are at your ſervice.

I am,

SIR,

Your obedient humble ſervant,

WILLIAM GREGOR.

Creed, near Grampound, Cornwall,
July 6th, 1803.

II. *The*

II.

The Method of making Artificial Yeaſt in Germany and Sweden. Communicated by* SIR A. N. EDELCRANTZ.

Artificial yeaſt. One hundred pounds of malt is brewed with hops nearly in the common way;—evaporated to half;—fermented with yeaſt;—and agitated with fifty or more pounds of meal.

TO one hundred pounds of the beſt malt, conſiſting of one part of malted wheat and two parts of malted barley, dried in the open air and well ground or bruiſed; add ten pounds of good hops, and brew the mixture with three hundred and fifty pounds of water to form wort, in the common way. After a ſhort boiling you ſeparate the grains and hops from the wort, which laſt by continued boiling may be reduced to the half or one hundred and ſeventy-five pounds. Cool it down as ſoon as poſſible to 70° fahrenht. and mix it then with thirty-two pounds of good yeaſt, which this firſt time may be common brewers yeaſt, but in every ſubſequent operation of the artificial. The wort will very ſoon ferment, and after four or five hours it will be covered with a thick, white, yeaſty froth. When this appears, the whole maſs muſt be ſtrongly agitated, and at the ſame time mix it well with from fifty to ſeventy-five pounds of fine ground meal of wheat or barley, either malted or unmalted. By keeping it conſtantly in a cool place, it will continue in a good ſtate for ten or fifteen days in ſummer, and in winter from four to ſix weeks, and ſtill longer if ſtirred once or twice daily. This yeaſt, when employed in diſtilleries, breweries, or for making cider and vinegar, as alſo by bakers and paſtry-cooks, is aſſerted to be as uſeful and effectual as the beſt common yeaſt.

III.

Remarkable Fact of the Diſappearance of a Mixture of Oxigen and Hydrogen, at the common Temperature over Water; inducing the Probability that Water may be formed at low Heats. In a Letter from Mr. B. HOOKE.

To Mr. NICHOLSON.

SIR,

IF you think the following fact which occurred to me in the ſummer of 1800 (and which ſeems rather to prove the forma-

* This proceſs was communicated to me by my learned friend, the celebrated chemiſt, Mr. Weſtrumb in Hamelm, and differs very little, though more perfect, from the method uſed in Sweden.

4 tion

tion of water from its elements at common temperatures) worthy a place in your very interefting Journal, the infertion will oblige, Sir,

your's, &c.

B. HOOKE.

Fleet Street, July 12, 1803.

HAVING been accuftomed to keep (for the purpofe of deflagrating) a mixture of oxigen and hidrogen gafes in the proportion as nearly as I could guefs that would form water, I was much furprifed upon inverting a quart bottle of fuch a mixture in a pneumatic tub, to find the water immediately rife up and fill it; as I could only account for it by fuppofing water had been formed from its conftituent parts, or that the gafes had efcaped from the bottle and left a vacuum. Either fuppofition appears to be attended with confiderable difficulties; for what nice play of the affinities could occafion the abftraction of that portion of caloric which is effential to the aëriform ftate of the gafes, and muft be before water could poffibly be formed; and on the other hand, if the gafes had made their efcape at the cork, what prevented the atmofpherical air from entering? The bottle containing the gafes had been left inverted in a common bottle rack with a fmall quantity of water in its neck for about three months.

Oxigen and hidrogen mixed and confined in an inverted bottle with a little water difappeared during three months and left a vacuum.

IV.

Experiments on the Quantity of Gafes abforbed by Water, at different Temperatures, and under different Preffures. By Mr. William Henry *.

THOUGH the folubility of an individual gas in water forms, generally, a part of its chemical hiftory, yet this property has been overlooked, in the examination of feveral fpecies of the clafs of aëriform fubftances. The carbonic acid, indeed, is the only gas whofe relation to water has been an object of much attention; and, at a very early period of its hiftory, Mr. Cavendifh, in the courfe of inquiries, the refults of which were the groundwork of the moft important fubfequent difcoveries, afcertained, with peculiar care, the proportion of carbonic acid

Solubility of gafes in water hitherto little examined,

except with regard to carbonic gas.

* From the Philofophical Tranfactions for 1803.

gas

gas condensible in water, at the temperature of 55° of Fahrenheit. Dr. Priestley also, about the same period, directed his attention to the saturation of water with fixed air, and contrived a simple and effectual mode of obtaining this impregnation. His apparatus, afterwards, gave way to the more manageable one of Dr. Nooth; and this, in its turn, has been superseded by the improved mode of condensing, into water, many times its bulk of various gases, invented and practised by several chemical artists, (as well as by myself,) both in this country and abroad.

Pressure.

The influence of pressure, in accomplishing this strong impregnation, was first, I believe, suggested by Dr. Priestley. "In an exhausted receiver," that most ingenious philosopher observes, "Pyrmont water will actually boil, by the copious discharge of its air; and I do not doubt, therefore, that by means of a condensing engine, water might be much more highly impregnated with the virtues of the Pyrmont spring *."

Before describing my experiments on the effects of additional pressure, in saturating water with gases, it will be necessary to state the results of others, that were previously expedient, to determine the quantity of each gas combinable with water, at a given temperature, and under the ordinary weight of the atmosphere. In a few instances, also, it was deemed proper to ascertain the influence of different temperatures, over the condensation of gases in water.

SECTION I.

ON THE QUANTITY OF GASES ABSORBED BY WATER, UNDER THE USUAL PRESSURE OF THE ATMOSPHERE.

Apparatus for shewing the proportion of gas absorbed by water.

In order to attain considerable minuteness in observing the proportion of gases absorbed by water, an apparatus was employed, of which the following is a description.

The vessel A (Plate I. Fig. 1.) is of glass, about 2 inches diameter, and 4½ inches long. It is graduated into cubical inches, and quarter inches; and furnished at the top with a brass cap, into which a cock *a* is screwed. To the lower aperture, a copper tube C is cemented, which is bent at a right angle, the leg nearest the vessel being carried downwards, and

* Experiments on Air, arranged and methodized, Vol. I. p. 51.

furnished

furniſhed with a cock *b*. B is a glaſs tube, of about ¼ inch bore, bent at a right angle, and graduated, from a given point, into hundredth parts of a cubical inch. It is attached to the copper pipe, by a tube of Indian rubber D, over which is a covering of leather, forming a joint, which admits of the veſſel A being briſkly agitated. When the apparatus is uſed, it is firſt filled with quickſilver; a transfer bottle of elaſtic gum, furniſhed with a cock, and containing water of a known temperature, is ſcrewed on; and a communication is opened, through the cocks, between the bottle and the glaſs veſſel. The lower cock *b* is then opened, through which the mercury runs out, while its place is ſupplied by a quantity of water from above, meaſurable by the ſcale on A. This transfer is removed, and another containing gas being ſubſtituted, a meaſured quantity of gas is admitted in a ſimilar manner. Strong agitation is now applied, by means of the joint D; and mercury is poured into the tube B, to ſupply the deſcent occaſioned by the abſorption in A; its level being exactly preſerved in both legs of the ſyphon, both at the commencement and cloſe of the experiment. The quantity of mercury required for this purpoſe, indicates preciſely the amount of the gas abſorbed.

The only advantage of this apparatus over a cylindrical jar, inverted in the uſual way over mercury, is, that by means of the tube B, very minute degrees of abſorption may be meaſured, which would ſcarcely be perceived in a wide veſſel.

Another apparatus for gaſes ſparingly abſorbed by water.

For the more abſorbable gaſes, I found this inſtrument to anſwer perfectly well, but, for aſcertaining the ſolubility of thoſe which are taken up by water in only ſmall proportion, I preferred one of different conſtruction. It conſiſted ſimply of a glaſs veſſel, of the capacity of 57½ cubical inches, and ſhaped as in Fig. 2. At *a* was cemented a cock, provided with a ſcrew; and the lower cock *b* was of glaſs, accurately ground in. The veſſel was then filled with water which had been long boiled; a lifting valve was ſcrewed on *a*, the cock being open, and the veſſel was placed under the receiver of an air pump, where it was kept for ſome time, the pump being occaſionally worked, as long as any air bubbles could be ſeen to ariſe. The gas under examination was next admitted from an elaſtic bottle, the cock *b* being opened, and a meaſured quantity of water let out. The gas and water were then violently agitated

agitated together; and the cock *b* opened under mercury, which afcended into the veffel. The agitation was ftill continued, obferving to preferve the fame level of mercury without as within the veffel; and, when it rofe no higher, the afcent was noted by means of the graduated fcale. The quantity of mercury that had entered the veffel, indicated the amount of abforption that had enfued.

Another apparatus, being fimply a glafs globe with a long neck.

It might, however, be objected, that the water would acquire air again, while poured into the veffel; and I therefore fometimes ufed large glafs globes, having long necks, accurately graduated. Thefe globes, being of very thin glafs, were filled with boiling water, and inverted inftantly in a trough of quickfilver. When the water became cold, the mercury was, of courfe, found to have rifen partly into the veffel. This portion was difplaced by a meafured quantity of gas; and the abforption was denoted by the afcent of the mercury in the graduated neck.

General proceedings and cautions.

The water employed in thefe experiments was boiled, during feveral hours, in a tin veffel having an aperture barely fufficient to allow the egrefs of the fteam, and poured, while boiling hot, into glafs veffels, which were corked, and tightly tied over with bladder. An equable temperature was produced in the water, mercury, and gas, except when above 85°, by regulating that of the room in which the experiments were made; and the glafs veffel, during agitation, was carefully guarded from the warmth of the hand. The agitation was continued, till it appeared, by the fcale, to produce no further effect; and, in the abforption of difficultly condenfible gafes, was repeated at intervals, during a fpace of from twelve to twenty-four hours. Alterations of the barometer were always obferved; and the refiduary gas meafured, or eftimated, at a preffure of $29\frac{1}{2}$ inches.

1. *Abforption of Carbonic Acid Gas by Water.*

Carbonic acid gas lefs abforbable by water at higher temperatures.

That the temperature of water influences the proportion of carbonic acid which it is capable of abforbing, is already known as a general fact; * but the exact amount of this influence has not, I believe, been hitherto afcertained. In the courfe of a feries

* See Mr. Cavendifh's experiments in the Phil. Tranf. Vol. LVI. p. 163; and Fourcroy's *Syfteme*, 4to. Tom. I. p. 215.

ſeries of experiments to determine it with preciſion, I was ſurpriſed by obtaining reſults which differed conſiderably from each other, at the ſame temperature of the gas and water; when both were, in different experiments, of like purity; and when the barometer had the ſame elevation. Of the cauſe of theſe variations I was not aware, till my friend Mr. Dalton ſnggeſted, that they probably depended on the variable amount of the reſidues; and, on repeating the experiments, with different proportions between the gas and the water, this ſuggeſtion was fully confirmed. Thus, when two meaſures of carbonic acid gas were agitated with one meaſure of water, the abſorption was conſiderably greater than when, to the ſame quantity of water, a leſs proportion of gas was uſed. The cauſe of this diminiſhed abſorption, ſeems to be connected with the proportion of common air contained in the unabſorbed reſiduum; for, beſides the unavoidable contamination of the gas employed, with a minute portion of the air of the veſſel uſed for its extrication, a ſmall quantity will always be liberated from the water, whatever pains have been taken to deprive it of air, by previous long boiling, expoſure under the air pump, or both in ſucceſſion. That this is the true explanation, appears alſo, from the reſult of adding to the gas a proportion of common air. Thus, when, at the temperature of 55°, 20 meaſures of carbonic acid are agitated with 10 of water, at leaſt 10 meaſures of gas are taken up; but, from a mixture of 20 meaſures of carbonic acid with 10 of common air, 10 parts of water take only 6 of carbonic acid, or 4 leſs than in the former inſtance.

The reſults at like temperatures and preſſures are not conſtant, unleſs like quantities of g s be preſented;

becauſe the ſmaller reſidues will contain a greater proportion of common air which retains gas along with it.

An analogous fact was obſerved by Dr. Brownrigg *, who remarked that gas does not eſcape from the water which it impregnates, unleſs the water be in contact with air: for, when the Pouhon water was excluded from air, but, at the ſame time, liberty was given for its gas to ariſe into an empty bladder, the gas did not ſpontaneouſly ſeparate from the water; but, on the contrary, remained united with it, when expoſed to the greateſt heat of our climate. When the impregnated water, he obſerves, is thus excluded from air, the gas will eſcape very ſlowly, at any temperature leſs than 110° of Fahrenheit, al-

Reverſe fact. That gas eſcapes much more readily from water when it can iſſue into common air.

* See Dr. Brownrigg's Paper on the Pouhon Water. Phil. Tranſ. Vol. LXIV.

though

though ſuch heat be ſufficient for the diſtillation of water ; nor can it be wholly expelled by a heat of 160° or 170°, continued two hours. But it is well known, that water ſaturated with carbonic acid gives up its gas rapidly, when freely expoſed to the atmoſphere.

In fixing the proportion of carbonic acid abſorbed, it is therefore neceſſary to note the quantity of reſiduum, as is done in the following table.

Tabulated reſults of carbonic acid abſorbed by water.

Experiment.	Temperature.	Meaſures of water.	Meaſures of gas.	Quantity abſorbed.	Reſidue.	Abſorbed by 100 inches of water.
1	55	13	32	14	18	108
2	85	13	32	11	21	84
3	55	13	24	14	10	108
4	55	10	15	10	5	100
5	55	20	20	18	2	90
6	55	19	19	16	3	84
7	85	19	19	13	6	70
8	110	10	20	6	14	60
9	110	20	20	9	11	45

Since the above table was drawn up, I have been gratified by remarking that, in the experiments of Mr. Cavendiſh, ſimilar variations in the quantities abſorbed, were produced by the variable amount of the reſidua ; as will appear from the following deductions from his experiments.

At the temperature of 55°.

Mr. Cavendiſh's reſults.

1. When the gas abſorbed was to the reſidue as 100 to 164, 100 cubical inches of water took up - - 116
2. When the abſorbed gas was to the reſidue as 100 to 16, 100 inches of water took up - - - 107
3. The abſorbed gas being to the reſidue as 100 to 10, 100 parts of water abſorbed - - - $102\frac{1}{2}$
4. The abſorption being to the reſidue as 100 to $1\frac{1}{2}$, 100 parts of water took up - - - - - $95\frac{1}{2}$

The quality of the reſiduum, I only aſcertained in experiments 5 and 6 of the preceding table. In experiment 5, the reſiduary two meaſures contained $7\frac{1}{2}$ *per cent.* of common air, or 0.15 of a meaſure. But, of thoſe, .13 exiſted previouſly in the 20 meaſures of carbonic acid gas; and the 20 meaſures

of

of water had, therefore, only given up .02 of a meafure, or about $\frac{1}{1500}$ of its bulk. I apprehend, however, that the whole of the common air was not, even thus, extricated from the water. In experiment 6, the 3 refiduary meafures contained $\frac{1}{6}$ of common air.

To judge of the influence of temperature, it is effential that the experiments compared fhould be on fimilar proportions of gas and water. Thus, from a comparifon of experiment 1 and 2, it appears, that about $\frac{1}{14}$ of the whole bulk abforbable at 55°, is the diminution of the quantity of abforption produced by each elevation of 10° of temperature; and the fame inference follows from various other experiments, the refults of which I have thought it needlefs to ftate *.

Carbonic acid is abforbed at 55° and 85° in quantities as 9 to 7.

2. Sulphuretted Hydrogen Gas.

One hundred parts of water, at 55° of temperature, abforb 86 parts of this gas, obtained from fulphuret of iron and dilute fulphuric acid, a refidue being left, equal in bulk to the gas abforbed. At 85°, under fimilar circumftances, the fame quantity abforbs 78.

Sulphuretted hydrogen gas is abforbed at 53° and 85°, in quantities as 10 to 9.

3. Nitrous Oxide.

At 45°, 100 cubic inches of water take up 50 of nitrous oxide; and, at 70°, the fame quantity takes up only 44. According to Mr. Davy, in whofe experiments, from his intimate knowledge of this gas, and fkill in its preparation, I place more confidence than in my own, 100 inches of water at 45°, take up 54 of nitrous oxide, the refiduum being about one half the volume of the gas abforbed.

Nitrous oxide is abforbed at 45° and 70°, in quantities as 8 to 7.

4. Lefs abforbable Gafes.

The experiments with thofe gafes which are abforbed only in fparing proportion by water, I could not conveniently make at more than one temperature; nor, indeed, did the object

Lefs abforbable gafes.

* During the abforption of carbonic acid by water, the gas and water having previoufly the fame temperature, there is an extrication of calcoric, fufficient to raife the temperature of the water between $\frac{1}{2}$ and $\frac{1}{4}$ of a degree of Fahrenheit. The fame effect is produced by the condenfation of fulphuretted hydrogen, and nitrous oxide gafes, though lefs apparently. To perceive this phenomenon, confiderable quantities of gas and water fhould be ufed.

appear

appear to me worthy of the time and attention which ſuch a repetition of them would have required. Of the accuracy of the following, however, I ſatisfied myſelf, by repeating each two or three times; and with gaſes of the greateſt attainable purity.

These numbers are corrected in the appendix.

100 cubic inches of water, at 60°, abſorb,

Of nitrous gas - - - - -	5	inches.
Oxygenous gas - - - -	2.63	
Phoſphuretted hydrogen ditto -	2.14	
Gaſeous oxide of carbon - -	2.01	
Carburetted hydrogen gas - -	1.40	
Azotic gas - - - - -	1.20	
Hydrogen gas - - - - -	1.08	

The ſolubility of atmoſpherical air cannot eaſily be aſcertained; for, as I ſhall hereafter ſhew, in a memoir on the expulſion of gaſes from water by each other, air is decompoſed by agitation with boiled water, its oxygenous portion being abſorbed in preference.

Former ſtatements of air ſeparable from water inaccurate from the portion ſtill left in the fluid.

From the ſtatements given by various philoſophers, (the Abbé Nollet, Drs. Hales, Prieſtley, and Pearſon,) of the quantity of air ſeparable from water of different kinds, by heat or a diminiſhed preſſure, I expected that a much larger proportion of the gaſes conſtituting the atmoſphere would have been abſorbed by water, than the above numbers aſſign. It is to be recollected, however, that no method hitherto diſcovered detaches from water all its air; and the unknown quantity remaining in it, after theſe modes of ſeparation have been employed, is to be added to that with which a given volume of water can be artificially impregnated. Dr. Pearſon, in his enquiries into the nature of the gas obtained by paſſing electric diſcharges through water, was at great pains to purify the ſubject of his experiments from air, by boiling and a powerful air pump; but he always found, that after the full effect of both theſe methods, electricity liberated a further, and not an inconſiderable, portion of air *.

Expulſion of gas from ſpring water.

Common ſpring water may, I think, be fairly taken as a ſpecimen of water full charged with atmoſpherical air; and, with the view of determining the quantity and kind of gaſes extricated from it, I made the following experiment. A glaſs

* Phil. Tranſ. for 1797.

globe

globe, of the capacity of 117½ cubical inches, was filled with water frefh from the well. To its mouth was adapted a curved and ftoppered tube, which held ¾ of an inch; and this was alfo filled with water. The globe was then placed in a veffel of brine, which was kept boiling between fix and feven hours; and the gafes were received over mercury. Their quantity and quality were as follows.

Its nature and quantity.

No.	Cub. inches.	Confifted of Carbonic acid.	Confifted of Air.	Proportion of Oxygen gas in the refiduary air.
1	1.25	0.50	0.75	0.20
2	1.25	0.85	0.40	0.16
3	1.63	1.23	0.40	0.16
4	0.50	0.49	0.01	loft by accident,
	4.63	3.07	1.56	
Air remaining in the bent tube.	0.75			
	5.38, total gas from 117½ inches of water.			

But, 4½ inches of water were expelled, owing to the expanfion by heat. Therefore 117½ − 4½ = 113 inches of water, gave 5.38 inches of gas; and 100 inches, confequently, gave 4.76, of which 3.38 were carbonic acid, and 1.38 atmofpherical air. Hence, the water afforded about $\frac{1}{70}$ its bulk of atmofpherical air, and $\frac{1}{20}$ of a mixture of gafes. In this eftimate, the gas remaining in the tube is reckoned as carbonic acid, which may be allowed, fince the portion laft obtained held only $\frac{1}{50}$ its bulk of common air.

SECTION II.

ON THE INFLUENCE OF PRESSURE IN PROMOTING THE ABSORPTION OF GASES; AND THE DESCRIPTION OF AN APPARATUS FOR EXHIBITING THIS PHENOMENON.

Method of operating when the gafes were abforbed under preffure.

For the purpofe of determining the ratio between the addition of preffure and the increafed abforption of gafes by water, I employed the apparatus, with fome addition, which has been already defcribed. The tube B was lengthened at pleafure, with the view of obtaining, by a column of mercury, any additional preffure that might be required. The veffel A, Fig. 1, was

was then filled completely with mercury, which rofe to its correfponding level in the tube B. A given quantity of water, of a known temperature, and afterwards a meafured volume of gas, were transferred into the veffel, in the mode already defcribed; and, as the mercury, by opening the cock *b*, was brought to the fame level in both legs of the fyphon, the gas, it is evident, muft have been under the ordinary weight of the atmofphere. A quantity of mercury was next poured into the leg B, fufficient to form a column 28 inches higher than the level of the mercury in A, after this addition; and the bulk of the gas was again noted. This was found to be, pretty exactly, $\frac{1}{2}$, $\frac{1}{3}$, &c. of the fpace occupied before, when one, two, or more additional atmofpheres were applied. Brifk agitation was now ufed, as long as any abforption took place; and, into the tube B an affiftant poured mercury, fo as to preferve in it the excefs of 28 inches above the level of the mercury in A. The degree of abforption was known by the fcale on A, or, more accurately, by the quantity of mercury required to fupport the elevation of 28 inches in B.

By lengthening the column in B to 56 inches, the preffure of two additional atmofpheres was obtained; and this was the utmoft extent to which the addition of weight could be carried, without forcing the joint at D.

When the cock *b* was opened, and the column in each leg thus fuddenly fell to the fame level, the water, which had been previoufly charged with gas, under a preffure of three atmofpheres, effervefced violently; but fome time elapfed before the additional gas, forced in by compreffion, was wholly evolved. Thefe appearances are very ftriking and amufing; and are well calculated for exhibition in a chemical lecture. The apparatus, however, I have no doubt, may be greatly improved; but, at the diftance of nearly 200 miles from the metropolis, I was under the neceffity of ufing fuch an one as could be conftructed by my own hands.

Improved apparatus.

A confiderable improvement in the conftruction of the apparatus, which would obviate the expediency of the flexible tube D, would be the following. To the lower neck of the veffel A, Fig. 1, let a cap and cock, with a female fcrew, be cemented; and let the upper end of the pipe C be terminated by a cock with a male fcrew. Introduce the gas and water, in

in the manner already defcribed ; apply the increafed preffure; and, having fhut the two additional cocks, unfcrew them from each other. The veffel A will thus be detached, and agitation may be eafily applied ; after which, again fcrew it into its former place, and, on opening the two cocks, the mercury will rife in the veffel A. Supply the defcent in B by frefh mercury, and proceed as before, repeating alternately the preffure and agitation, as long as any further abforption takes place.

A further amendment of the apparatus, would confift in the fubftitution of cocks of fome other metal than brafs, which, however perfect at firft, are always injured by the repeated action of the mercury. If cocks of glafs could be ground fufficiently tight, metal caps with fcrews might be cemented to them.

Operation under preffure with the lefs abforbable gafes.

For obferving the increafed abforption of lefs condenfible gafes, I found it neceffary to fubftitute a veffel of larger fize than A, and of the capacity of at leaft 50 cubical inches. It is reprefented by the dotted lines in Fig. 1, and was furnifhed with a cock and fcrew at *c*. As it would have been troublefome to have filled fo large a veffel entirely with quickfilver, it was filled with boiled water, with the exception of a quantity of quickfilver rather exceeding the bulk of the gas employed. The gas was admitted, as ufual, from a transfer bottle, the mercury which it replaced efcaping through the cock *b*. The increafed preffure was next applied ; and the experiment conducted as before, except that the agitation was much longer continued.

General law. Water at like temperatures always abforbs the fame *bulk* of gas, however denfe or rare,

The refults of a feries of at leaft fifty experiments, on carbonic acid, fulphuretted hydrogen gas, nitrous oxide, oxygenous and azotic gafes, with the above apparatus, eftablifh the following general law: *that, under equal circumftances of temperature, water takes up, in all cafes, the fame volume of condenfed gas as of gas under ordinary preffure.* But, as the fpaces occupied by every gas are inverfely as the compreffing force, it follows, *that water takes up, of gas condenfed by one, two, or more additional atmofpheres, a quantity which, ordinarily compreffed, would be equal to twice, thrice, &c. the volume abforbed under the common preffure of the atmofphere.* By frequent repetion of the experiments, I obtained refults differing a little from

and confequently a *mafs* or *quantity* proportional to the preffure.

from the general principle above ſtated; but, for all practical purpoſes, I apprehend, the law has been announced with ſufficient accuracy *.

Increaſed temperature of gas by condenſation.

In place of the cock *a*, I cemented, in one experiment, a very ſenſible thermometer. The veſſel was next filled with mercury through the cock *b*; and the tube B being alſo filled, the cock *b* was ſhut, and a bottle of carbonic acid gas ſcrewed on. The cock *b* being then opened, the mercury deſcended, and a meaſured quantity of carbonic acid aroſe into the veſſel A. In the ſame way, a meaſured quantity of water was introduced. When the denſity of the air was ſuddenly doubled by a column of quickſilver, the mercury in the thermometer, whoſe bulb was ſtill ſurrounded by the condenſed gas, roſe about $1\frac{1}{2}$ degree. On agitating the veſſel, till the water encompaſſed the bulb of the thermometer, an elevation of barely $\frac{1}{2}$ a degree enſued in the temperature of the water. This aſcent would probably have been greater, if the evolved heat had not been carried off by the mercury on which the water floated.

Mancheſter, Dec. 8th, 1802.

APPENDIX.—(Since read.)

Corrections of the numbers in the foregoing paper.

SINCE my paper was finiſhed I have found that the numbers aſſigned in it, as indicating the quantities taken up by water of ſome of the more abſorbable, and of all of the leſs abſorbable gaſes, are rather below the truth. The accuracy of theſe numbers I was led to doubt by a ſuſpicion that due attention had not always been paid, in my former experiments, to the quality of the unabſorbed reſiduum. For, the theory which Mr. Dalton has ſuggeſted to me on this ſubject, and which appears to be confirmed by my experiments, is that the abſorption of gaſes by water is purely a mechanical effect, and that its amount is exactly proportional to the denſity of the gas, conſidered abſtractedly from any other gas with which it may accidentally be mixed. Conformably to this theory, if the reſiduary gas contain $\frac{1}{2}$, $\frac{1}{10}$, or any other proportion of foreign gas, the quantity abſorbed by water will be $\frac{1}{2}$, $\frac{1}{10}$, &c. ſhort of the maximum. The proof of theſe propoſitions would lead me into a minuteneſs of detail not ſuited to the preſent occaſion; I therefore haſten to communicate the reſults of my lateſt experiments.

Abſorption of gaſes is purely mechanical.

The

The report which I have already given of the quantity of carbonic acid gas abforbed under the ordinary preffure of the atmofphere I find no reafon to correct, but of fulphurated hydrogen gas I have effected a larger abforption than the one before ftated, and have repeatedly obferved its amount to be 106 or 108 by 100 meafures of water, at 60° of Fahrenheit, which temperature is to be underftood in all the following experiments.

On the correct quantities of the feveral gafes abforbed.

Of feveral experiments on the abforption of nitrous oxide, I take the following as a fair example of the whole. I agitated at three feveral times, 1175 meafures of nitrous oxide, with 1320 meafures of water; 1025 parts of gas difappeared, and the unabforbed remainder 150 contained 15 of foreign admixtures. It follows that 100 parts of water had taken up 77.6 of nitrous oxide; and after adding to thefe the diminution of abforption occafioned by the impurity of the refidium, it may be inferred that 100 parts of water would abforb 86 of abfolutely pure nitrous oxide.

With refpect to the remaining gafes I have been prevented by urgent profeffional engagements, from examining the quantity of each abforbable under fimilar circumftances, except in the inftances of oxygenous, azotic and hydrogenous gafes. The refults of thefe experiments are comprifed in the following table. The firft column fhows the quantity of gas which 100 parts of water, at 60°, have actually abforbed; the fecond the quantity which ought to be taken up, provided the refidue were in a ftate of abfolute purity. In the example of nitrous gas alone, the eftimated is lefs than the actual abforption; becaufe a fmall portion of this gas lofes its ærial form, by union with the oxygenous gas from which water cannot be entirely freed.

A table fhewing the quantity of each gas abforbed by 100 meafures of water at 60°.

Table of abforptions of gafes.

	Actual Abforption.	Inferred Abforption.
Nitrous gas - - -	5. - - -	5.
Oxygenous gas - -	3.55 - - -	3.7
Phofphuretted hydrogen gas	2.14 - - -	
Gafeous oxide of carbon -	2.01 - - -	
Carburetted hydrogen gas	1.40 - - -	
Azotic gas - - - -	1.47 - - -	1.53
Hydrogenous gas - -	1.53 - - -	1.61

V.

Of the State of Vapour ſubſiſting in the Atmoſphere. By RICHARD KIRWAN, ESQ. LLD. F. R. S. *and* P. R. I. A*.

(*Concluded from Page* 215.)

MR. SAUSSURE, *Hygrometer*, p. 284, has given us the ſpecific gravity, not indeed of pure vapour, but of vapour diſſolvedin air, with more preciſion, for he tells us, 1. That a cubic foot of perfectly dry air has its volume augmented by $\frac{1}{54}$ when ſaturated with ten grains of moiſture at about 65° Fahr. of heat, and barometer 28,77 inches (Engliſh).

Specific gravity of dry air and of moiſt air.

2. That a cubic foot of *pure* or perfectly dry air of that denſity and at that temperature weighs 751 grains (French), and after diſſolving 10 grains of moiſture, by which it is dilated $\frac{1}{54}$, this new volume weighs 751 + 10 = 761 grains; but a cubic foot of *pure* air augmented by an acceſſion of $\frac{1}{54}$ of its bulk of pure air would weigh 751 + $\frac{1}{54}$ 751 = 765 grains, that is 14 grains more. Hence he infers that in this caſe the ſpecific gravity of the diſſolved moiſture is to that of dry air as 10 to 14, for $\frac{1}{54}$ of a cubic foot in the one caſe weighs 10 grains, and in the other 14 grains nearly.

Suſpected errors in the original experiment.

But I ſtrongly ſuſpect that the original experiment, on which this calculation is founded, is erroneous, chiefly by reaſon of the ſtrong adherence of moiſture to cold glaſs, as will hereafter be ſeen in treating of dew. From Schmidt's experiments, it may be inferred, that the ſpecific gravity of vapour diſſolved in air at this temperature is much lower with reſpect to that of pure air than Sauſſure has ſtated, for he tells us that about 1066 meaſures of dry air in temperature 65° would, if ſaturated with moiſture at that temperature, occupy the ſpace of about 1100 meaſures, and conſequently receive an augmentation amounting to about $\frac{1}{32}$ of their bulk; now, transferring this ratio to the cubic foot in Sauſſure's experiment, it appears that $\frac{1}{32}$ of a cubic foot thus added to the cubic foot of dry air weighs 10 grains; but a cubic foot of dry air, augmented by an acceſſion of $\frac{1}{32}$ of ſimilar air, would weigh 751 + 23,46 grains, which approaches nearly to Mr. Watt's ratio, therefore the ſpecific gravity of vapour diſſolved in air at this temperature is to that of perfectly dry air as 10 to 23,5 nearly. It ſhould however be

be recollected that Mr. Sauſſure found that a cubic foot of dry air in reality took up 11,069 grains of moſture when ſaturated at this temperature, and that it was only by way of conceſſion to thoſe againſt whom he argued, that he ſtated the weight taken up at 10 grains; then we ſhould have of 11,069 to 21,195, or in round numbers as 11 to 21 or 10 to 19. And it ſhould farther be remarked, that the temperature is given very looſely, for it is ſtated to be from 14 to 15 or 16 degrees of Reaumur. See *Hygrometer*, p. 104 and 284.

Table of the quantities of vapour in air of different denſities but like temperature.

Sauſſure has given us a table, by the help of which the abſolute quantity of vapour at any barometrical height, in a cubic foot of air being known, the proportion and abſolute quantity in a cubic foot, at another barometrical height, 3,6 inches lower, may be known from the mercurial height 28,77 to that of three inches and one-half, nearly.

This table I here give, adapting it to our meaſures.

Barometer	Ratio.
28,77	1,0000
25,17	0,9528
21,57	0,8899
17,97	0,8264
14,37	0,7629
10,77	0,6887
7,17	0,6230
3,57	0,4311

Thus ſuppoſing the abſolute quantity of diſſolved vapour at any temperature, and barometer 28,77 to be 10 grains per cubic foot, then the quantity of vapour at a height at which a barometer would ſtand at 25,17 inches would be $10 \times 0{,}9528 = 9{,}528$ and at the height at which a barometer would ſtand at ſeven inches, the quantity in a cubic foot would be only $10 \times ,6230 = 6{,}23$. But ſtill it is ſuppoſed that at thoſe great heights, at which barometers would ſtand ſo low, that the air is of the ſame temperature as the original experiment is made at, namely in this caſe, as it is found at barometer 28,77 inches; but ſince in reality air at great heights is generally much colder than below, to aſcertain the real proportion of vapour at thoſe heights it will be neceſſary to find the quantity of vapour which a cubic foot of air is capable of holding at that temperature barometer 28,77, and the ratio which the quantity or weight of vapour actually found bears to the complement at that temperature. Then, 2. to find the complement of a cubic foot of air at the temperature which prevails at the given barometrical height, and diminiſh it in the ſame ratio in which it was found diminiſhed below, and finally diminiſh it ſtill farther in the ratio which that barometrical height demands. An example will fully explain this rule.

— correction for different temperatures.

Thus Sauſſure found, barometer 28,77 and thermometer 82° of Fahr. a cubic foot of air contained about 10 grains of moiſture at Geneva. Now the complement of 82° is nearly 15 grains, and the ratio of 10 to 15 is $\frac{2}{3}$. Then at Mount Blanc, on the ſame hour, the barometer ſtood at 16° and the thermometer at 26°,8; the complement of a cubic foot of air at this temperature is 5,3 grains, which diminiſhed in the ratio of 2 to 3 becomes 3,5, and this, farther diminiſhed by the ratio which the barometrical height of 16 inches demands, namely $,78 = 3,5 \times ,78 = 2,7$ grains, by obſervation it was found to be 1,7; the difference is only one grain. *Voy. aux Alpes*, § 2007. How the temperature which prevails at thoſe great heights may be found, will be ſhewn in the ſequel.

Syſtem of Lambert reſpecting the quantities of vapour in air of different denſities.

The celebrated Lambert, of Berlin, Mem. Berlin, 1772, has alſo given an eſtimate of the proportion of vapour which prevails in the atmoſphere at different barometrical heights, deduced from calculations founded on many fictions, ſuch as that of an homogeneous atmoſphere, of pure air diſtinct from common air, and an erroneous ſyſtem of the aſcent of heat; yet as it is much eaſier in its application and in the inſtance juſt quoted, approaches very near the truth, I have calculated the reſults of his ſyſtem, which is nothing more than that the quantity of vapour at different barometrical heights above the earth is in the ratios of the ſquares of thoſe heights. By an homogeneous atmoſphere it is probable he meant ſuch a ſtate of the atmoſphere as prevails in ſerene unclouded weather, and it is certainly only in ſuch an atmoſphere that any calculation can be inſtituted.

Table of the Ratios of the Quantities of Vapour at different barometrical Heights, the Quantity of the Surface of the Earth being given.

Barometer.	Ratio of Vapour.	Barometer.	Ratio of Vapour.	Barometer.	Ratio of Vapour
30,	900	24,	576	12	144
29,5	870	23,	529	11	121
29,	841	22	484	10	100
28,5	812	21	441	9	81
28,	784	20	400	8	64
27,5	756	19	361	7	49
27,	729	18	324	6	36
26,5	702	17	289	5	25
26,	676	16	256	4	16
25,5	650	15	225	3	9
25,	625	14	196	2	4
24,5	600	13	169	1	1

Thus

Thus in the example laſt quoted, the quantity of vapour in a cubic foot at Geneva being 10 grains, barometer 28,77, the quantity on Mount Blanc, barometer 16, ſhould be ,309, for as 827,7 ($=\overline{28,77^2}$) is to 256 ($=\overline{16^2}$) ſo is 10 to 0,309, which differs from the truth by only 0,391 of a grain.

As vapours unite to air, partly through the agency of heat, and partly through that of affinity and of electricity, ſo they ſeparate from it, ſometimes from a diminution of that degree of heat which they poſſeſſed in their naſcent ſtate, ſometimes from a diminution of affinity, and ſometimes from an alteration in their electrical ſtate.

Separation of vapour from air by change of heat and of electricity.

In their firſt degree of coaleſcence when ſeparated from air, they form aggregrates of exceeding minute particles, ſeparated from air by the diminution of affinity, and alſo from each other by electrical atmoſpheres; theſe aggregates are of equal, and often lower, ſpecific gravity, than the air in which they are formed, and yet are viſible by reaſon of their opacity; when near the earth they are called *foggs*, *miſts* or *haze*, (which differ only in denſity) and when at greater heights, *clouds*.

Firſt coaleſcence of vapour, miſts or clouds.

Vapours iſſuing from water or moiſture warmer than the air to which they unite, are ſoon cooled by it and thence in great meaſure diſmiſſed; hence the morning miſts obſerved in ſummer and the winter miſts of the colder regions; evening miſts on the contrary proceed from the ſuperſaturation of air with vapours previouſly diſſolved, ariſing from the ſupervening decreaſed temperature. The inferior ſtrata of the atmoſphere are ſcarce ever ſuperſaturated by vapours ariſing from water or moiſture warmer than the air into which they aſcend; for before the point of ſaturation can be attained, their affinity to the portion of air to which they are united is weakened, and thence exceeded by the unincumbered affinity of the ſuperior ſtrata, and this happens ſucceſſively on to the higher regions; but with diminiſhed activity, by reaſon of the diminiſhed denſity of the higher ſtrata, until their ulterior progreſs is checked by ſaturation; but as they are ſtill continually recruited from below, their quantity is at laſt ſo far increaſed that they coaleſce into clouds. Here the proceſs recommences, for from the ſurface of theſe clouds a freſh evaporation often takes place, which, after ſome progreſs, is again checked in its turn, and clouds are formed at a ſuperior height; theſe again give room to a further evaporation, and a new ſtage of clouds is formed, until the

Statement of the manner in which clouds are formed.

the procefs is at laft arrefted by the intenfe cold of the fuperior regions. But the mere cold of congelation is not fufficient to arreft it, for Bouguer informs us that clouds are formed 2500 feet above the lower line of congelation, and that ice itfelf evaporates, though cooled, feveral degrees below the freezing point, is well known. The diftance of the particles, both of air and vapour, from each other, when fo far rarified as they muft be in the fuperior regions of the atmofphere prevents their coalefcence in any but the extreme degrees of cold.

Clouds in ftrata above each other. Hence we fee that in the warmer latitudes and feafons, various ftrata of clouds may be formed one above the other; Mufchenbrook attefts, that even in Holland, in Auguft 1748, he diftinctly difcerned three. Thefe diftinct ftrata, varioufly electrified and otherwife circumftanced, give occafion to various phænomena, the detail of which would here be mifplaced.

Clouds near mountain tops. The clouds which commonly crown the fummits even of low mountains, and often announce rain, are caufed by the near approach to faturation, at thofe elevations, and its actual attainment through the evaporation from thofe fummits. But the fummits of the loftieft mountains ever crowned with fnow, are generally fhrouded in clouds from the cold they impart to the air in contact with them, and the lofs of electricity conducted away from the vapours contained in that air, by the mountain.

Heights at which clouds are formed. The heights at which the loweft clouds are formed are various in various latitudes and feafons; greater in the warmer and fmaller in the colder. In latitude 54° in Cumberland, Mr. Crofthwaite obferved none lower than 2700 feet, and none higher than 3150 in the courfe of feveral years *. But this country being mountainous they are probably lower than in others under the fame parallel. Lambert, in Berlin, latitude 52° 32′, in the month of July 1773, found their height 7792 feet, thermometer 65°, and the barometer fomewhat below its mean height †. Schuckburg alfo remarks, that clouds frequently reft below the fummit of *Saleve,* whofe height is 2831 feet. Phil Tranf. 1777, p. 538, and Gentil at Pondicherry, latitude 12°, obferved fome at the height of 10240 feet. 2 Voy. p. 79.

* D'Alton's Meteorological Obfervations, p. 41.

† Mem. Berlin, 1773, p. 44.

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The weight of clouds, Sauſſure eſtimates at one-third or one-fourth of that of the cubic foot of air in which they ſubſiſt. Hygrometer, p. 270. When the barometer riſes, clouds are partly diſſolved, as denſe air is a better ſolvent than rarer air, and partly riſe higher in conſequence of the increaſed ſpecific gravity of the inferior air; when the barometer falls the contrary takes place.

Clouds weigh about one third of the air they float in.

VI.

Experiments and Obſervations in ſupport of that Theory of Ventriloquiſm which is founded on the Reflection of Sound. In a Letter from Mr. JOHN GOUGH.

To Mr. NICHOLSON.

SIR,

YOU have given a ſhort but comprehenſive view of my theory of ventriloquiſm, at page 202, of the fourth volume of your Journal; after which, you obſerve, that the manifeſt difficulty of managing the echos of a room renders my explanation of the phenomenon doubtful. In order to examine the force of this objection experimentally, I proceeded in the following manner: perhaps the inſtruments which were uſed on the occaſion, may be deemed childiſh by many of your readers; but a genuine friend to phyſical inquiry looks for the event of an experiment, and does not regard the appearance of the apparatus.

Whether the phenomenon of ventriloquiſm be referable to echo.

Exp. 1. I took the ſhell of a cocoa-nut, one end of which had been cut off, ſo as to make it an aperture, ſomething more than two inches in diameter: after lining this with woollen cloth, I put a watch into it, and ſurrounded the whole with a pillow in a manner, that left no part of the ſhell in contact with the atmoſphere, except the open end. When things were thus prepared, the pillow was placed upon my knees, having the mouth of the nut turned from me: I then liſtened to the ſound of the watch, ſitting in an erect poſture, and obſerved it to be weak, but diſtinct as to direction: a circular metal tray, one foot in diameter, was held up before my face, in the next place, and at the diſtance of two feet from it; upon which, the clicking of the watch grew much ſtronger,

Exp. 1. The beats of a watch made to iſſue chiefly in one direction. This ſtrongeſt found was then reflected by a metallic ſurface and was referred to its laſt direction.

ſtronger, and proceeded from the tray. The certainty of the latter circumſtance was aſcertained by changing the place of the reflector, ſo as to make the ſound fall upon my right and left ears alternately.

Exp. 2. A ſmall metallic rattle was ſubſtituted inſtead of the watch; and other reflectors tried: with ſimilar reſults.

Exp. 2. The reſult of the laſt experiment convinced me of the practicability of turning the echos of a room to the uſe of a ventriloquiſt, ſeeing I had employed thoſe of my own lap to a ſimilar purpoſe; nevertheleſs I deſired to give an additional proof of the ſame thing, by the aſſiſtance of a ſtronger ſound. With this view, I took a ſmall metal rattle, belonging to a child's toy; it weighed 25 grains, and conſiſted of two thin convex ſhells of braſs, ſomething leſs than one inch broad: a pellet was lodged in the cavity formed by the junction of theſe hollow plates; the motion of which produced a ſound, ſuch as I wanted. This rattle being put into the ſame-cocoa nut prepared as in the laſt experiment, I gave a gentle motion to it by my finger, which was introduced at the aperture; and I ſoon perceived, that the ſound might be transferred in this caſe alſo, from the ſhell to the tray, but not with equal certainty: for frequently a ſlight change, given to the inclination of the reflector, rendered the place of the ſound evident, which was indiſtinct before; and the contrary. The reaſon of this uncertainty will be inveſtigated hereafter; and I may add at preſent, that a thin quarto as well as a fan were ſubſtituted with ſucceſs, in lieu of the tray; but bodies of three or four inches in diameter, did not produce the effect. The preceding experiments have been related with an attention to circumſtances, which cannot fail of inſtructing any one to repeat them accurately; and a few trials will, without doubt, convince him who will take the trouble, that the echos of a room are ſubject to external influence, as well as thoſe of a valley.

Obſervations on the facts. The reflected ſound was made to predominate over, and obliterate the direct ſound, &c.

We come in the next place to examine the peculiarities that gave riſe to theſe counterfeit ſounds; that is the unuſual circumſtances of my experiments are to be reviewed, and their effects aſcertained. The ſhell was encompaſſed by a bad conductor of ſound on all ſides, the mouth excepted; it was from this aperture then, that the pulſes entered the atmoſphere with their wonted freedom. Now we know from experience, that confined ſound does not diverge from the place of its egreſs, as it were from a centre: for ſhould any one attend to

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a noiſe coming from an open window, he always finds it ſtrongeſt, in front of the paſſage; ſounds therefore reſemble the properties of a river entering a lake; the current of which affects its former direction, after it is freed from the reſtraints of a channel. My ear was placed behind the apparatus, in relation to the ſhell's mouth; conſequently it did not lay in the track of the direct pulſes; therefore the ſound, heard by me in the abſence of the reflector, ſhall be ſuppoſed at preſent to have conſiſted wholly of lateral pulſes, reflected from the circumference of the aperture; but when a greater maſs of ſound was forced on the ear, from a different quarter, the weaker became imperceptible; which is a thing, that happens invariably in ſimilar caſes; this change in the direction of the prevailing pulſes neceſſarily transferred the ſeat of the ſound from the ſhell to the reflector. But if the reflected lateral pulſes can be ſilenced in one caſe, by diverting thoſe of the centre towards the ſame parts; a like rule might be expected to obtain in all caſes: becauſe the powers of reflected ſounds may be ſuppoſed to have a fixed relation to their primitives; but the rule fails in a number of inſtances; conſequently my artificial ventriloquiſt poſſeſſes another ſource of ſound; which is the next ſubject of inquiry.

The ſame effect appears univerſally producible.

Exp. 3. When I let the ſmall rattle fall from the height of an inch, upon the cloth which lined the ſhell the ſtroke was heard in the nut; but the noiſe, occaſioned by the rebounding of the pellet, came from the reflector; in this inſtance then, we have two ſounds, proceeding from different places, though they evidently iſſued from the ſame ſpot; and the cauſe, that gave them oppoſite directions, may be inveſtigated in the following manner. The ſtroke of the rattle gave vibrations to the ſhell, which were ſtronger than thoſe imparted by the pellet; the former ſet of vibrations could not be ſuppreſſed by the pillow on account of their ſtrength, they therefore produced a ſound, conjointly with the lateral pulſes, which proved an overmatch for the echo of the reflector; while the feebler force of the ſecond ſet was overpowered by the pulſes, reflected from the tray. In reality the art of ſuppreſſing the direct ſound appears to conſtitute the ſecret of ventriloquiſm; for the more completely this ſound is ſmothered, the eaſier it is to place the reflector properly: this appears to be the reaſon, why the experiment ſucceeds with a watch more frequently than

Exp. 3. The rattle was let fall in its confining receptacle. The ſtroke on the receptacle gave a direct ſound; the rattle in its rebound gave the reflex ſound:

The ſecret of ventriloquiſm is to ſuppreſs the direct ſound.

than with the rattle; becaufe the impulfe of the former is lefs than that of the latter. In fact, if the force of the founding body be augmented, while the power of the muffle remains unchanged, the reflector foon becomes ufelefs; as I found by ufing a fmall bell, the found of which was difguifed with the greateft difficulty in my rude apparatus; and every precaution, that could be taken made it a hard tafk to place the reflector properly. The beft pofition of this body is apparently that which brings the ear in the line, conftituting the path o the ftrongeft pulfes, after they are reflected; which line and their courfe before reflection muft make equal angles at the reflecting furface, with a perpendicular to it; which muft alfo lay in the fame plane with them. I have now gone through the demonftration of a propofition, which forms the foundation of my theory of ventriloquifm; fhould you think proper to repeat the preceding experiments, I only afk the indulgence of a little patience on your part: treat them in other refpects with critical accuracy; it was my intention to fcrutinize your theory freely; but too much of the prefent fheet is occupied already.

JOHN GOUGH.

Middlefhaw, July 16, 1803.

REPLY. W. N.

I never meant to have expreffed a doubt as to the interefting facts refpecting found, which Mr. Gough has ftated and commented upon, in his paper at page 125 of our fecond volume; nor can it be difputed that many curious effects are produced by the reflection and tranfmiffion of found. I am very far from wifhing to advance any theory of an art concerning which I have witneffed fo little; and I take it for granted that the ingenious author, when he fpeaks of his theory and mine, in the laft paragraph of the preceding communication, does not mean to fupport the pofition that the effects of what is called ventriloquifm are exclufively produced by reflection. The narrative of Fitz James's performance at page 202. vol. 4. of our Journal, will at leaft prove that the caufes which I have confidered rather as moral than phyfical, muft be admitted in many cafes, if not in all. Poffibly there may be ventriloquifts who operate more by the reflection of found, than by the art of

The ventriloquifm of Fitz James was an imitation of founds with appropriate action and not reflection.

of imitation affifted by theatrical action; as Fitz James evidently does, to a fcattered audience, who cannot all, or even many of them, be fuppofed to be duly placed for receiving one and the fame echo: and upon a careful recollection of the pofitions, I am much inclined to think none of them were fo placed. The reflection of found by a fimple echo,—its more complicated echo from a curved furface, or whifpering gallery, or fpeaking trumpet,—its yet unexplained tranfmiffion along a fmooth wall or the furface of water,—its tranfmiffion through or along the ground, fo well known to militarymen, and to which the ftring of Dr. Moyfe *, the fpeaking apparatus of Ezekiel Walker †, and other fimilar effects, are referable. Thefe and other modes of operation, unknown to or unthought of by me, may furely be ufed in producing delufions and other ufeful or remarkable effects, and may conftitute part of the art of ventriloquifm. I can only exprefs my opinion, that the art of the profeffor I have fpoken of was exquifite mimicry, and nothing more.

Many phenomena of found may produce delufion; but how far ventriloquifm?

VII.

Examination of the fo called Salt of Bitumen, the Bit-Nobin of the Hindoos. By FREDERICK ACCUM, *Practical Chemift and Teacher of Chemiftry. From the Author.*

IN the LV Number of Mr. Tilloch's Philofophical Magazine, p. 206, we find fome obfervations concerning a faline compound lately imported from India, under the name of *Salt of Bitumen* or *Bit-Nobin.* From the experiments which the author of that paper (Mr. Henderfon), made in order to become acquainted with the nature of that falt, it appeared to him to be chiefly compofed of common falt and fulphurated hidrogen gas. But as the experiments of this gentleman do not appear to have been made with a view of affording an analyfis of this faline fubftance, I was called upon by fome medical practitioners of eminence to examine chemically the nature of this curious compound. I fhall therefore confine myfelf merely to this examination, which was conducted in the following manner.

Account of Bit-Nobin, in the Philofophical Magazine, fuppofed to be common falt and fulphurated hidrogen.

* Philof. Journal. † Ibid.

Upon

Alcohol by digestion dissolved a portion;

Upon 480 grains of the salt reduced to a fine powder, 3ozs. of highly rectified alcohol were poured; the mixture was frequently agitated in a stopped vial, during the course of one week, and the alcohol afterwards separated by filtration; the salt which remained on the filtre was treated in a similar manner, with fresh quantities of alcohol repeatedly. The different portions of spirit employed which were all colourless, had on being mingled together a disagreeable odour and bitterish taste: they were evaporated to dryness.

which afforded after evap. and deliques. a white salt and yellowish liquid.

On leaving the product obtained in this manner unobserved to the air for some weeks, it was found to have attracted moisture so as to be separated into two distinct saline substances, namely, a yellowish fluid, and a crystalline white salt.

The yellow fluid being separated by alcohol was diluted with water and the alcohol driven off. Carbonate of soda threw down chalk; and,

To ascertain the nature of these saline bodies, alcohol was affused upon it in order to separate the liquid part from the crystallized salt. The fluid which was taken up by the alcohol, I mingled with a like quantity of water, heated the mixture to ebullition, and then dropt into it a solution of carbonate of soda. A cloudiness appeared and a precipitate was deposited, which after being washed and dried was found to be carbonate of lime. The residuary fluid from which this precipitate had been separated yielded nothing but muriate of soda.

hence the salt contains muriate of lime;

I learned from these experiments that the salt under examination contained muriate of lime.

the muriatic acid of which gave a precip with solution of silver, from the weight of which the quantity of muriate of lime was determined:

To determine the quantity of it contained in the above portion of salt, I judged it necessary to ascertain the quantity of one of its constituent parts; for the composition of this salt being known, the real quantity of the compound might be thence found. The artificial muriate of soda produced before was therefore dissolved in water, and its solution decomposed by the addition of nitrate of silver: the muriate of silver produced when perfectly dried, weighed 40 grains, which indicate nine grains of muriatic acid, answering to 12 grains of muriate of lime. This therefore was the true quantity of muriate of lime contained in the above quantity of salt.

The solid salt left by the alcohol was dissolved in water. It gave odour of sulphurated hidrogen, became green, and afforded a black precipitate.

Upon the salt which had been repeatedly extracted by alcohol, distilled water was poured in order to dissolve it. On shaking the mixture, the whole acquired instantly a dark olive green colour, the peculiar odour of sulphurated hidrogen gas was evolved and a black pulverulent precipitate became diffused

fused through the solution. To collect this precipitate the solution was filtred as expeditiously as possible: the precipitate washed and dried weighed six grains.

This precipitate to which the cause of the green colour of the solution was owing, after having been slowly dried, on exposure to air, had the appearance of a dark brown powder. It was soluble in muriatic, sulphuric and nitric acids. Tincture of galls and prussiate of potash proved it to be an oxide of iron. In order to be positive in this respect, a like quantity of the same precipitate obtained in a similar manner, was dissolved in sulphuric acid, and the solution again decomposed by carbonate of soda. The carbonate of iron was re-dissolved in dilute nitric acid, and repeatedly boiled to dryness in that fluid, till it became completely insoluble, and acquired all the properties of red oxide of iron. this was iron.

The saline solution from which this oxide had been separated, had a strong odour of sulphurated hidrogen gas. Its taste greatly resembled Harrowgate water. Mercury agitated in contact with it lost its brilliancy, and became covered with a black pellicle. Arsenious acid immersed into it became yellow; leaf silver tarnished instantly; nitrous acid, holding much nitrous gas in solution, occasioned a white precipitate; nitrate of silver, acetite of lead, nitrate of bismuth, occasioned black precipitates; muriate of barytes rendered it milky, and a precipitate was deposited which was insoluble in muriatic acid; prussiate of potash and tincture of galls had no effect upon it. All these experiments (the latter three excepted) sufficiently evinced the presence of sulphurated hidrogen gas. The fluid contained sulphurated hidrogen.

In order to ascertain the contents of the water it was evaporated to dryness: the salt obtained after having been examined in the usual manner, consisted entirely of muriate of soda, contaminated with a small quantity of sulphuric acid; its weight amounted to 444 grains. To ascertain the quantity of sulphuric acid, the salt was redissolved in water, and muriate of barytes was added till no further precipitate ensued. The sulphate of barytes produced when dry, weighed $51\frac{1}{2}$ grains, which are equivalent to 14 grains of sulphur. It is perhaps needless to remark that, the sulphuric acid thus found did not previously exist in the salt: its production was evidently owing to the decomposition of the produced sulphurated hidrogen gas, for no vestige of such acid could be detected by any other agent, or under other circumstances. The saline portion was muriate of soda with a small portion of sulphuric acid. The quantity of this last acid was found by the precipitate by muriate of barytes. The Sulphuric acid was not originally such, but was produced from sulphur;

Sulphur

which exifted in a fulphuret of iron in the falt.

Sulphur was therefore the compound which muft have been prefent; and if we are permitted to reafon from the phenomena which refult from the action of water upon this compound, it becomes obvious that the fulphur is joined to the iron, detected before, in the form of a fulphuret of iron. Hence it becomes evident that while the falt continues in a dry ftate, the equilibrium of the affinities of its conftituent parts remains unaltered;

The equilibrium of affinity is broken by the addition of water.

but the moment we bring this compound in contact with water, a new arrangement of principles takes place; the water is decompofed, the oxide of iron is reduced to a ftate of *minimum* of oxidation, and appears under the form of a black powder diffufed through the fluid, fulphuric acid is formed by the decompofition of the fulphurated hidrogen gas, and thus a fet of bodies appears which did not previoufly exift in the ftate wherein they prefent themfelves to our view.

Component parts of bit-nobin about 1 iron; 3 fulphur; $2\frac{1}{2}$ muriate of lime; $92\frac{1}{2}$ common falt.

It is evident therefore that 480 grains of this falt yielded by this analyfis.

Black oxide of iron	- - -	6 grains.
Sulphur	- - - - - -	14
Muriate of lime	- - - - -	12
Muriate of foda	- - - -	444
		476
Lofs	- - -	4
		480

How to determine the fulphurated hidrogen if required.

I have taken no notice in this analyfis of the quantity of fulphurated hidrogen gas evolved during the folution of a given quantity of falt in a limited quantity of water, this being foreign to my fubject. I merely remark that, if this is wanted to be afcertained, it may eafily be found by introducing it into a glafs tube clofed at the top, and furnifhed at the other extremity with a ftopper, a weighed quantity of falt, and then adding to it a meafured quantity of water, taking care to fill the tube with this fluid no higher than about $\frac{2}{3}$. Having done this let the tube be ftopped, and agitate it till all the falt is diffolved, or at leaft till no more gas is extricated. Then let the tube be immerfed into warm water, and let it ftand undifturbed: remove the ftopper carefully fo as to get rid of the depofited oxide of iron, and introduce into the faline folution tube, nitrous gas, in fmall quantities, till no further diminution of the gas in the upper part of the tube takes place, or till no more

red

red vapours are produced by a new addition of a fmall quantity of the gas. The faline folution will now be turbid: leave it therefore undifturbed, and a precipitate will be depofited, which is fulphur, from the quantity of fulphur fo produced we may learn the quantity of fulphurated hidrogen gas which was contained in the folution, for one grain of it when perfectly dry, are equivalent to 3.33 cubic inches of fulphurated hidrogen gas.

Old Compton Street, Soho,
July 20, 1803.

VIII.

Experiments on the Invifible Rays of the Solar Spectrum. By M. RITTER, *of Jena**. *Communicated by* M. VICKTRED, *Doctor in the Univerfity of Copenhagen, to the Editors of the Bulletin des Sciences at Paris,* No. 73.

THESE enquiries form a fequel to the experiments by which Herfchell difcovered the exiftence of invifible calorific rays beyond the limits of the folar fpectrum. The experiments of M. Ritter prefent a very fimple method of proving the exiftence of thefe rays by the exhibition of a very curious property which he fays is peculiar to them.

Ritter's experiments on invifible rays.

He placed muriate of filver without the folar fpectrum and next to the violet rays. This oxide became blackened in a fhort time, it became ftill deeper in the violet rays, ftill more in the blue, and fo on.

The rays in the confine of the violet reduce or blacken the muriate of filver,

On the contrary, placing muriate of filver a little blackened next to the red rays, and without the fpectrum it quickly became white, that is to fay, it was difoxigenated.

thofe near the red render it white or difoxide it.

According to M. Ritter, thefe experiments may be performed very well with phofphorus: by letting fall on it the invifible ray neareft to the red, it inftantly emits white vapours; but the invifible rays neareft the violet are thrown on the fame phofphorus, it inftantly becomes extinguifhed.

Combuftion and extinction of phofphorus by the fame means.

From thefe facts, M. Ritter concludes, that there exifts without the fpectrum, and at its two extremities, invifible rays which poffefs the property of affifting oxigenation and difoxigenation.

* From the Bulletin des Sciences, Germinal, An. XI.

The

Negative galvanifm gives the fenfation of red; pofitive of blue.

The fame philofopher has alfo obferved a fingular coincidence between thefe effects and thofe of galvanifm. He finds that when the eye is placed in contact with the negative conductor of the pile, it fees every object red, but if placed againft the pofitive conductor it fees them blue: whence there appears to refult an analogy between the action of the negative electricity and that of the red light, and of the pofitive and the violet light.

IX.

An Account of fome Experiments and Obfervations on the Conftituent Parts of certain Aftringent Vegetables; and on their Operation in Tanning. By HUMPHRY DAVY, Efq. *Profeffor of Chemiftry in the Royal Inftitution**.

Seguin's difcovery of the tanning matter;

THE difcovery made by M. Seguin, of a peculiar vegetable matter which is effential to the tanning of fkin, and which is poffeffed of the property of precipitating gelatine from its folutions, has added confiderably to our knowledge of the conftituent parts of aftringent vegetables.

extended by Prouft;

Mr. Prouft has inveftigated many of the properties of this fubftance; but, though his labours, and thofe of other chemifts, have led to various interefting obfervations, yet they are far from having exhaufted the fubject. The affinities of tannin have been hitherto very little examined; and the manner in which its action upon animal matters is modified by combination with other fubftances, has been fcarcely at all ftudied.

purfued by the author at the defire of the Royal Inftitution.

At the defire of the Managers of the Royal Inftitution, I began, in September 1801, a feries of experiments on the fubftances employed in the procefs of tanning, and on the chemical agencies concerned in it. Thefe experiments have occupied, ever fince, a confiderable portion of my leifure hours; and I now prefume to lay before the Royal Society an account of their general refults. My chief defign was, to attempt to elucidate the practical part of the art; but in purfuing it, I was neceffarily led to general chemical inquiries concerning the analyfis of the different vegetable fubftances containing tannin, and their peculiar properties.

* From the Philofophical Tranfactions, 1803.

I. OBSERVATIONS ON THE ANALYSIS OF ASTRINGENT VEGETABLE INFUSIONS.

The ſubſtances that have been ſuppoſed to exiſt moſt generally in aſtringent infuſions are, tannin, gallic acid, and extractive matter.

Aſtringents contain tannin, gallic acid and extractive.

The preſence of tannin in an infuſion, is denoted by the precipitate it forms with the ſolution of glue, or of iſinglaſs. And, when this principle is wholly ſeparated, if the remaining liquor gives a dark colour with the oxygenated ſalts of iron, and an immediate precipitate with the ſolutions of alum and of muriate of tin, it is believed to contain gallic acid, and extractive matter.

Tannin ſhewn by glue, gallic acid, and extract, by ſalts of iron, and by alum and muriate of tin.

The experiments of MM. Fourcroy, Vauquelin, and Seguin, have ſhown that many aſtringent ſolutions undergo a change by expoſure to the atmoſphere; an inſoluble matter being precipitated from them. A precipitation is likewiſe occaſioned in them by the action of heat; and theſe circumſtances render it extremely difficult to aſcertain, with any degree of preciſion, the quantities of their conſtituent parts, as they exiſt in the primitive combination.

Aſtringent ſolutions very apt to change.

After trying ſeveral experiments on different methods of aſcertaining the quantity of tannin in aſtringent infuſions, I was induced to employ the common proceſs of precipitation by gelatine, as being the moſt accurate.

This proceſs, however, requires many precautions. The tanning principle in different vegetables, as will be ſeen hereafter, demands for its ſaturation different proportions of gelatine; and the quantity of the precipitate obtained by filtration, is not always exactly proportional to the quantities of tannin and gelatine in ſolutions, but is influenced by the degree of their concentration. Thus, I found that 10 grains of dry iſinglaſs, diſſolved in two ounces of diſtilled water, gave, with ſolution of galls in exceſs, a precipitate weighing, when dry, 17 grains; whilſt the ſame quantity, diſſolved in ſix ounces of water, produced, all other circumſtances being ſimilar, not quite 15 grains. With more diluted ſolutions, the loſs was ſtill greater; and analogous effects took place, when equal portions of the ſame ſolution of iſinglaſs were acted on by equal portions of the ſame infuſion of galls diluted in different degrees

Precip. of tannin by gelatine *(a)* varies in different vegetables; *(b)* and by the concentration of the ſolutions.

grees with water; the leaſt quantity of precipitate being always produced by the leaſt concentrated liquor. In all caſes, when the weak ſolutions were uſed, it was obſerved, that the reſidual fluid, though paſſed two or three times through the filtre, ſtill remained more or leſs turbid and opaque; ſo that it is moſt likely that the deficiency aroſe from the continued ſuſpenſion of ſome of the minutely divided ſolid matter in the liquid maſs.

The ſolutions of gelatine muſt be quite freſh and very ſtrong;

The ſolutions of gelatine, for the purpoſes of analyſis, ſhould be employed only when quite freſh, and in as high a ſtate of ſaturation as is compatible with their perfect fluidity. I have obſerved, that in caſes when they approach towards the ſtate of jelly, their power of acting upon tannin is materially altered, and they produce only a very ſlight precipitation. As the degree of fluidity of ſolutions of gelatine is influenced by their temperature, I have found it expedient, in all comparative experiments, to bring them and the aſtringent infuſions on which they are deſigned to act, as nearly as poſſible to a common degree of heat. My ſtandard temperature has been between 60 and 70° Fahrenheit; and the ſolutions of gelatine that I have uſed, were made by diſſolving 120 grains of iſinglaſs in 20 ounces of water.

at the temperature 60°.

If an exceſs of gelatine be added ſome of the tannin will be rediſſolved.

In aſcertaining the proportions of tannin in aſtringent infuſions, great care muſt be taken to prevent the preſence of any exceſs of gelatine; for, when this exceſs exiſts, I have found that a ſmall portion of the ſolid compound formed is rediſſolved, and the reſults of the experiment otherwiſe affected. It is not difficult to diſcover the preciſe point of ſaturation, if the ſolution of iſinglaſs be added only in ſmall quantities at a time, and if portions of the clear liquor be paſſed through a filtre at different periods of the proceſs. The properties of theſe portions will indicate the quantities of the ſolution of gelatine required for the completion of the experiment.

The compariſons of the quantities of precipitant and precipitate made after drying at 150°.

That the compoſition of any precipitate containing tannin and gelatine may be known with a tolerable degree of preciſion, it is neceſſary that the iſinglaſs employed in the ſolution, and the new compound formed, be brought as nearly as poſſible to the ſame degree of dryneſs. For this purpoſe, I have generally expoſed them, for an equal time, upon the lower plate of a ſand-bath, which was ſeldom heated to more than

150°

150°. This method I have found much better than that of drying at the temperatures of the atmosphere, as the different states of the air, with regard to moisture, materially influence the results.

Mr. Hatchett has noticed, in his excellent Paper on Zoophytes, &c.* that isinglass is almost wholly composed of gelatine. I have found, that 100 grains of good and dry isinglass contain rather more than 98 grains of matter soluble in water. So that, when the quantity of isinglass, in any solution employed for acting upon astringent infusion, is compared with the quantity of the precipitate obtained, the difference between them will indicate the proportion of tannin, as it exists in the combination.

Isinglass is gelatine nearly pure.

After the tannin has been separated from an astringent infusion, for the purpose of ascertaining its other component parts, I have been accustomed to evaporate the residual liquor very slowly, at a temperature below 200°.† In this process, if it contains extractive matter, that substance is in part rendered insoluble, so as to fall to the bottom of the vessel. When the fluid is reduced to a thick consistence, I pour alcohol upon it. If any gallic acid or soluble extractive matter be present, they will be dissolved, after a little agitation, in the alcohol; whilst the mucilage, if any exist, will remain unaltered, and may be separated from the insoluble extract, by lixiviation with water.

After precipitating the tannin, the fluid is partly evaporated, and the gallic acid and soluble extract taken up by spirit.

I have made many experiments, with the hope of discovering a method by which the respective quantities of gallic acid and extractive matter, when they exist in solution in the alcohol, may be ascertained; but without obtaining success in the results. It is impossible to render the whole of any quantity of extractive matter insoluble by exposure to heat and air, without at the same time decomposing a portion of the gallic

It is difficult to ascertain the proportions of gallic acid and extract in the alcohol.

* Phil. Trans. for 1800, page 327.

† M. Deyeux has shewn, (*Annales de Chimie*. Tome XVII. page 36,) that in the process of evaporating solutions of galls, no gallic acid is carried over by the water, at a temperature below that of ebullition. Many astringent infusions, however, lose a portion of their aromatic principle, even in cases when they are not made to boil; but this substance, though evident to the smell, in the water that comes over, cannot be detected by chemical reagents.

 acid.

acid. That acid cannot be ſublimed, without being in part deſtroyed; and, at the temperature of its ſublimation, extractive matter is wholly converted into new products.

Ether takes the former in preference, but has little effect as to perfect ſeparation.

Ether diſſolves gallic acid; but it has comparatively little action upon extractive matter. I have been able, in examining ſolutions of galls, to ſeparate a portion of gallic acid by means of ether. But, when the extractive matter is in large quantities, this method does not ſucceed, as, in conſequence of that affinity which is connected with maſs,*, the greateſt part of the acid continues to adhere to the extract.

Alumine attracts extract, but does not ſeparate it perfectly from gallic acid.

Alumine has a ſtrong attraction for extractive matter; but comparatively a weak one for gallic acid †. When carbonate of alumine is boiled for ſome time with a ſolution containing extractive matter, the extractive matter is wholly taken up by the earth, with which it forms an inſoluble compound; but, into this compound, ſome of the gallic acid appears likewiſe to enter; and the portion remaining diſſolved in the ſolution is always combined with alumine.

This problem not yet perfectly ſolved.

I have not, in any inſtance, been able to ſeparate gallic acid and extractive matter perfectly from each other; but I have generally endeavoured to form ſome judgment concerning their relative proportions, by means of the action of the ſalts of alumine, and the oxygenated ſalts of iron. Muriate of alumine precipitates much of the extractive matter from ſolutions, without acting materially upon gallic acid; and, after this precipitation, ſome idea may be formed concerning the quantity of the gallic acid, by the colour it gives with the oxygenated ſulphate of iron. In this proceſs, however, great care muſt be taken not to add the ſolution of the ſulphate of iron in exceſs; for, in this caſe, the black precipitate formed with the gallic acid will be rediſſolved, and a clear olive-coloured fluid only will be obtained.

Approximation.

The ſaline parts of aſtringent infuſions are alſo ſcarcely to be ſeparated.

The ſaline matters in aſtringent infuſions, adhere ſo ſtrongly to the vegetable principles, that it is impoſſible to aſcertain their nature with any degree of accuracy, by means of common re-

* See Berthollet, *Récherches ſur les Lois de l'Affinité. Mém. de l'Inſtitut National.* Tome III. p. 5.

† See Fiedler, *Journal de Chimie, par* J. B. Van-mons, Tome I. page 85.

agents.

agents. By incineration of the products obtained from the evaporation of aſtringent infuſions, I have uſually procured carbonate of lime and carbonate of potaſh.

In the different analyſes, as will be ſeen from the reſults given in the following ſections, I have attended chiefly to the proportions of the tanning principle, and of the principles precipitable by the ſalts of iron, as being moſt connected with practical applications.

The practical reſults are by precipitation with gelatine and with ſalts of iron.

With regard to the knowledge of the nature of the different ſubſtances, as they exiſt in the primitive aſtringent infuſion, we can gain, by our artificial methods of examination, only very imperfect approximations. In acting upon them by reagents, we probably, in many caſes alter their nature; and very few of them only can be obtained in an uncombined ſtate. The compariſon, however, of the products of different experiments with each other, is always connected with ſome uſeful concluſions; and the accumulation of facts with regard to the ſubject, muſt finally tend to elucidate this obſcure but moſt intereſting part of chemiſtry.

Though ſtrict analyſis is here very difficult, yet the experiments lead to uſeful concluſions.

II. EXPERIMENTS ON THE INFUSIONS OF GALLS.

I have been very much aſſiſted in my inquiries concerning the properties of the infuſions of galls, by the able Memoir of M. Deyeux, on galls.*

The ſtrongeſt infuſion of galls that I could obtain, at 56° Fahrenheit, by repeatedly pouring diſtilled water upon the beſt Aleppo galls broken into ſmall pieces, and ſuffering it to remain in contact with them till the ſaturation was complete, was of the ſpecific gravity 1.068. Four hundred grains of it produced, by evaporation at a temperature below 200°, fifty-three grains of ſold matter; which, as well as I could eſtimate, by the methods of analyſis that have been juſt deſcribed, conſiſted of about $\frac{9}{10}$ of tannin, or matter precipitable by gelatine, and $\frac{1}{10}$ of gallic acid, united to a minute portion of extractive matter.

Cold Infuſion of 400 grains of galls gave 53 grains of ſolid matter, which was nine parts tannin and one gallic acid.

100 grains of the ſolid matter obtained from the infuſion, left, after incineration, nearly 4½ grains of aſhes; which were chiefly calcareous matter, mixed with a ſmall portion of fixed alkali.

The infuſion reddened litmus. Its ſolid matter left by incineration alone 1-20th chiefly calcareous with ſome alkali.

* *Annales de Chimie*, Tome XVII. page 1.

alkali. The infufion ftrongly reddened paper tinged with litmus. It was femitranfparent, and of a yellowifh-brown colour. Its tafte was highly aftringent.

Sulph. acid precipitates the foton.

When fulphuric acid was poured into the infufion, a denfe whitifh precipitate was produced; and this effect was conftant, whatever quantity of the acid was ufed. The refidual liquor, when paffed through the filtre, was found of a fhade of colour deeper than before. It precipitated gelatine, and gave a dark colour with the oxygenated fulphate of iron.

This precip. feemed to be the acid with tannin and gallic acid and fome extract.

The folid matter remaining on the filtre, flightly reddened vegetable blues; and, when diffolved in warm water, copioufly precipitated the folutions of ifinglafs. M. Prouft,* who firft paid attention to its properties, fuppofes that it is a compound of the acid with tannin: but I fufpect that it alfo contains gallic acid, and probably a fmall portion of extractive matter. This laft fubftance, as is well known, is thrown down from its folutions by fulphuric acid; and I found, in diftilling the precipitate from galls by fulphuric acid, at a heat above 212°, that a fluid came over, of a light yellow colour, which was rendered black by oxygenated fulphate of iron; but which was not altered by gelatine.

Muriatic acid threw down a precip. of tannin and acid, and a fimilar compound with excefs of acid continued diffolved.

Muriatic acid produced, in the infufion, effects analogous to thofe produced by fulphuric acid; and two compounds of the acid and the vegetable fubftances were formed: the one united to excefs of acid, which remained in folution; the other containing a confiderable quantity of tannin, which was precipitated in the folid form.

Nitric acid deftroyed both principles and formed a kind of extract.

When concentrated nitric acid was made to act upon the infufion, it was rendered turbid; but the folid matter formed was immediately diffolved with effervefcence, and the liquor then became clear, and of an orange colour. On examining it, it was found that both the tannin and the gallic acid were deftroyed; for it gave no precipitate, either with gelatine or the falts of iron, even after the refidual nitric acid was faturated by an alkali. By evaporation of a portion of the fluid, a foft fubftance was obtained, of a yellowifh-brown colour, and of a flightly fourifh tafte. It was foluble in water, and precipitated the nitro-muriate of tin, and the nitrate of alu-

* The fact of the precipitation of folution of galls by acids, was noticed by M. Dizé. See *Annalès de Chimie*, Tome XXXV. p. 37.

mine;

mine; ſo that its properties approached to thoſe of extractive matter; and it probably contained oxalic acid, as it rendered turbid a ſolution of muriate of lime.

When a very weak ſolution of nitric acid was mixed with the infuſion, a permanent precipitate was formed, and the reſidual liquor, examined by the ſolution of gelatine, was found to contain tannin. Weak nitric acid acted more like the other acids.

A ſolution of pure potaſh was poured into a portion of the infuſion. At firſt, a faint turbid appearance was perceived; but, by agitation, the fluid became clear, and its colour changed from yellow brown to brown red; and this laſt tint was moſt vivid on the ſurface, where the ſolution was expoſed to the atmoſphere. The ſolution of iſinglaſs did not act upon the infuſion modified by the alkali, till an acid was added in exceſs, when a copious precipitation was occaſioned. Pure potaſh ſeemed to have combined with the tannin, which did not then ſeparate by gelatine till an acid had taken the alkali.

The compound of potaſh and ſolution of galls, when evaporated, appeared in the form of an olive-coloured maſs, which had a faint alkaline taſte, and which ſlowly deliqueſced when expoſed to the air. The alkaline compound ſeparated by evaporation.

Soda acted upon the infuſion in the ſame manner as potaſh; and a fluid was formed, of a red-brown colour, which gave no precipitate to gelatine. Soda acted like potaſh.

Solution of ammonia produced the ſame colour as potaſh and ſoda, and formed ſo perfect an union with the tannin of the infuſion, that it was not acted upon by gelatine. When the compound liquor was expoſed to the heat of boiling water, a part of the ammonia flew off, and another part reacted upon the infuſion, ſo as to effect a material change in its properties. A conſiderable quantity of inſoluble matter was formed; and the remaining liquor contained little tannin and gallic acid, but a conſiderable portion of a ſubſtance that precipitated muriate of tin, and the ſalts of alumine. Ammonia.

When the experiment on the ebullition of the compound of the infuſion and ammonia was made in cloſe veſſels, the liquor that came over was ſtrongly impregnated with ammonia; its colour was light yellow, and, when ſaturated with an acid, it was very little altered by the ſalts of iron. The reſidual fluid, after the proceſs had been continued for ſome time, as in the other caſe, precipitated gelatine ſlightly, but the ſalts of alumine copiouſly; and it gave a tinge of red to litmus paper.

When

Lime, ſtrontia, or barytes, in exceſs, threw down nearly the whole contents.

When ſolution of lime, of ſtrontia, or of barytes, was poured in exceſs into a portion of the infuſion, a copious olive-coloured precipitate was formed, and the ſolution became almoſt clear, and of a reddiſh tint. In this caſe, the tannin, the gallic acid, and the extractive matter, ſeemed to be almoſt wholly carried down in the precipitates; as the reſidual fluids, when ſaturated by an acid, gave no precipitate to gelatine, and only a very ſlight tint of purple to oxygenated ſulphate of iron.

Effect of ſmaller doſes.

When the ſolutions of the alkaline earths were uſed only in ſmall quantities, the infuſion being in exceſs, a ſmaller quantity of precipitate was formed, and the reſidual liquor was of an olive-green colour; the tint being darkeſt in the experiment with the barytes, and lighteſt in that with the lime. This fluid, when examined, was found to hold in ſolution a compound of gallic acid and alkaline earth. It became turbid when acted on by a little ſulphuric acid; and, after being filtrated, gave a black colour with the ſolutions of iron, but was not acted upon by gelatine.

Effect of lime more particularly ſtated,

When a large proportion of lime was heated for ſome time with the infuſion, it combined with all its conſtituent principles, and gave, by waſhing, a fluid which had the taſte of lime-water, and which held in ſolution only a very ſmall quantity of vegetable matter. Its colour was pale yellow; and, when ſaturated with muriatic acid, it did not precipitate gelatine, and gave only a ſlight purple tinge to the ſolutions of the ſalts of iron. The lime in combination with the ſolid matter of the infuſion, was of a fawn colour. It became green at its ſurface, where it was expoſed to the air; and, when waſhed with large quantities of water, it continued to give, even to the laſt portions, a pale yellow tinge.

alſo of magneſia.

Magneſia was boiled in one portion of the infuſion for a few hours; and mixed in exceſs with another portion, which was ſuffered to remain cold. In both caſes, a deep green fluid was obtained, which precipitated the ſalts of iron, but not the ſolutions of gelatine; and the magneſia had acquired a grayiſh-green tint. Water poured upon it became green, and acquired the properties of the fluid at firſt obtained. After long waſhing, the colour of the magneſia changed to dirty yellow; and the laſt portions of water made to act upon it were pale yellow, and altered very little the ſolutions of iron.

 When

When the magnefia was diffolved in muriatic acid, a brownifh and turbid fluid was obtained, which precipitated gelatine and the oxygenated falts of iron. So that there is every reafon to believe, that the earth, in acting on the aftringent infufion, had formed two combinations; one containing chiefly gallic acid, which was eafily foluble in water; the other containing chiefly tannin, which was very difficultly foluble.

Muriatic folution of magnefia.

Alumine boiled with the infufion became yellowifh-gray, and gave a clear white fluid, which produced only a tinge of light purple in the folutions of iron. When the earth * was employed in very fmall quantity, however, it formed an infoluble compound only with the tannin and the extract; and the refidual liquor was found to contain a gallate of alumine with excefs of acid.

Alumine.

The oxides of tin and of zinc, obtained by nitric acid, were boiled with feparate portions of the infufion for two hours. In both cafes, a clear fluid, which appeared to be pure water, was obtained; and the oxides gained a tint of dull yellow. A part of each of them was diffolved in muriatic acid. The folution obtained was yellow: it copioufly precipitated gelatine; and gave a denfe black with the falts of iron. Mr. Prouft †, who firft obferved the action of oxide of tin upon aftringent infufions, fuppofes that portions of tannin and gallic acid are decompofed in the procefs, or converted, by the oxigen of the oxide, into new fubftances. Thefe experiments do not, however, appear to confirm the fuppofition.

Effect of the oxides of tin and zinc on the infufion. They feized the diffolved matter, and a muriatic folution of the compound precipitated gelatine and gave a black with iron.

M. Deyeux obferved, that a copious precipitation was occafioned in infufion of galls, by folutions of the alkalis combined with carbonic acid. Mr. Prouft has fuppofed that the folid matter formed is pure tannin, feparated from its folution by the ftronger affinity of the alkali for water; and he recommends the procefs, as a method of obtaining tannin.

The precipitate by alkalis from inf. galls is not pure tannin as Prouft fuppofes;

In examining the precipitate obtained by carbonate of potafh fully combined with carbonic acid, and ufed to faturation, I have not been able to recognize in it the properties which are ufually afcribed to tannin: it is not poffeffed of the aftringent tafte; and it is but flightly foluble in cold water, or in alcohol.

for it is not aftringent, nor well foluble in cold water or alcohol, nor acts on gelatine, nor tans fkins.

* Mr. Fiedler, I believe, firft obferved the action of alumine upon tannin. Van-Mons's Journal, Vol. I. p. 86.

† *Annales de Chimie*, Tome XLII. p. 69.

Its

Its ſolution acts very little upon gelatine, till it is ſaturated with an acid; and it is not poſſeſſed of the property of tanning ſkin.

It contains alkali.

In various caſes, in which the greateſt care was taken to uſe no exceſs, either of the aſtringent infuſion or of the alkaline ſolution, I have found the ſolid matter obtained poſſeſſed of analogous properties: and it has always given, by incineration, a conſiderable portion of carbonate of potaſh, and a ſmall quantity of carbonate of lime.

The remaining fluid containing alkali,

The fluid remaining after the ſeparation of the precipitate, was of a dark-brown colour, and became green at the ſurface, when it was expoſed to the air. It gave no precipitate to ſolution of gelatine; and afforded only an olive-coloured precipitate with the ſalts of iron.

exhibited tanin when the alkali was ſaturated with mur. acid.

When muriatic acid was poured into the clear fluid, a violent effervеſcence was produced; the fluid became turbid; a precipitate was depoſited; and the reſidual liquor acted upon gelatine and the ſalts of iron, in a manner ſimilar to the primitive infuſion.

Cryſtals of gallic acid by diſtill. from the precipitate.

M. Deyeux, in diſtilling the precipitate from infuſion of galls by carbonate of potaſh, obtained cryſtals of gallic acid. In following his proceſs, I had ſimilar reſults; and a fluid came over, which reddened litmus-paper, and precipitated the ſalts of iron black, but did not act upon gelatine.

The precip. was partly ſoluble in warm water:

When the precipitate by carbonate of potaſh was acted upon by warm water, applied in large quantities, a conſiderable portion of it was diſſolved; but a part remained, which could not in any way be made to enter into ſolution; and its properties were very different from thoſe of the entire precipitate. It was not at all affected by alcohol: it was acted on by muriatic acid, and partially diſſolved; and the ſolution precipitated gelatine and the ſalts of iron. It afforded, by incineration. a conſiderable portion of lime, but no alkali.

Hence the precip. was tanin and gallic acid with a little alkali, and alſo the ſame with lime; both carbonated.

In comparing theſe facts, it would ſeem, that the precipitate from infuſion of galls, conſiſts partly of tannin and gallic acid united to a ſmall quantity of alkali, and partly of theſe vegetable matters combined with calcareous earth; and it will appear probable, when the facts hereafter detailed are examined, that both the potaſh and the lime are contained in theſe compounds in a ſtate of union with carbonic acid.

The

The ſolutions of carbonate of ſoda and of carbonate of ammonia, both precipitated the infuſion of galls in a manner ſimilar to the carbonate of potaſh; and each of the precipitates, when acted on by boiling water, left a ſmall quantity of inſoluble matter, which ſeemed to conſiſt chiefly of tannin and carbonate of lime.

Habitudes of carb. of ſoda and ammonia nearly as thoſe of potaſh.

The entire precipitate by carbonate of ſoda produced, when incinerated, carbonate of ſoda and carbonate of lime. The precipitate by carbonate of ammonia, when expoſed to a heat ſufficient to boil water, in a retort having a receiver attached to it, gave out carbonate of ammonia, (which was condenſed in ſmall cryſtals in the neck of the retort,) and a yellowiſh fluid, which had the ſtrong ſmell and taſte of this volatile ſalt. After the proceſs of diſtillation, the ſolid matter remaining was found of a dark brown colour; a part of it readily diſſolved in cold water, and the ſolution acted on gelatine.

Particular detail. The precipitate;

The reſidual fluid of the portions of the infuſion which had been acted on by the carbonates of ſoda and of ammonia, as in the inſtance of the carbonate of potaſh, gave no precipitate with gelatine, till they were ſaturated with an acid; ſo that, in all theſe caſes, the changes are ſtrictly analogous.

The reſidual fluid.

The infuſion of galls, as appears from the analyſis, contains in its primitive ſtate calcareous matter. By the action of the mild alkalis, this ſubſtance is precipitated in union with a portion of the vegetable matter, in the form of an inſoluble compound. The alkalis themſelves, at the ſame time, enter into actual combination with the remaining tannin and gallic acid; and a part of the compound formed is precipitated, whilſt another part remains in ſolution.

Infuſion of galls contains calcareous matter.

When the artificial carbonates of lime, magneſia, and barytes, were ſeparately boiled with portions of the infuſion of galls for ſome hours, they combined with the tannin contained in it, ſo as to form with it inſoluble compounds; and, in each caſe, a deep green fluid was obtained, which gave no precipitate to gelatine, even when an acid was added, but which produced a deep black colour in the ſolutions of the ſalts of iron.

Carbonates of alkaline earths gave inſol. precip.

The fluid ſhewed no tanin.

Sulphate of lime, when finely divided, whether natural or artificial, after having been long heated with a ſmall quantity of the infuſion, was found to have combined with the tannin of it, and to have gained a faint tinge of light brown. The liquid became

Sulphate of lime ſeized the tanin by boiling.

became of a blue-green colour, and acted upon the falts of iron, but not upon gelatine; and there is every reafon to fuppofe, that it held in folution a triple compound, of gallic acid, fulphuric acid, and lime.

Solutions of neutral falts precip. Inf. galls, but it is not pure tanin.

We owe to Mr. Prouft, the difcovery that different folutions of the neutral falts precipitate the infufion of galls; and he fuppofes, that the precipitation is owing to their combining with a portion of the water which held the vegetable matter in folution. In examining the folid matters thrown down from the infufion, by fulphate of alumine, nitrate of potafh, acetite of potafh, muriate of foda, and muriate of barytes, I found them foluble, to a certain extent, in water, and poffeffed of the power of acting upon gelatine. From the products given by their incineration, and by their diftillation, I am however inclined to believe that they contain, befides tannin, a portion of gallic acid and extractive matter, and a quantity of the falt employed in the primitive folution.

Metallic folutions give denfe precip.

It is well known, that many of the metallic folutions occafion denfe precipitates in the infufion of galls; and it has been generally fuppofed, that thefe precipitates are compofed of tannin and extractive matter, or of thofe two fubftances and gallic acid, united to the metallic oxide; but, from the obfervation of different proceffes of this kind, in which the falts of iron and of tin were employed, I am inclined to believe, that they contain alfo a portion of the acid of the faline compound.

probably containing fome of the acid.

Muriate of tin.

When the muriate of tin was made to act upon a portion of the infufion, till no more precipitation could be produced in it, the fluid that paffed through the filtre ftill acted upon gelatine and feemed to contain no excefs of acid; for it gave a precipitate to carbonate of potafh, without producing effervefcence. The folid compound, when decompofed by fulphuretted hydrogen, after the manner recommended by Mr. Prouft, was found ftrongly to redden litmus-paper, and it copioufly precipitated nitrate of filver: whereas, the primitive infufion only rendered it flightly turbid; fo that there is every reafon to believe, that the precipitate contained muriatic acid,

Oxigenated fulphate of iron.

By paffing the black and turbid fluid, procured by the action of folution of oxigenated fulphate of iron in excefs upon a portion of the infufion, through finely-divided pure flint, contained

tained in four folds of filtrating paper, I obtained a light olive-green fluid, in which there was no excefs of fulphuric acid, and which I am inclined to fuppofe was a folution of the compound of gallic acid and fulphate of iron, with fuperabundance of metallic falt. I have already mentioned that gallic acid, when in very fmall proportion, does not precipitate the oxigenated falts of iron; and Mr. Prouft, in his ingenious paper upon the difference of the falts of iron, has fuppofed that, in the formation of ink, a portion of the oxide of iron in union with gallic acid is diffolved by the fulphuric acid of the fulphate. This comes near to the opinion that they form a triple compound: and, in reafoning upon the general phenomena, it feems fair to conclude, that, in the cafe of the precipitation of tannin by the falts of tin and of iron, compounds are formed, of tannin and the falts; and that, of thefe compounds, fuch as contain tin are flightly foluble in water, whilft thofe that contain iron are almoft wholly infoluble.

In examining the action of animal fubftances upon the infufion of galls, with the view of afcertaining the compofition of the compounds of gelatine, and of fkin, with tannin, I found that a faturated folution of gelatine, which contained the foluble matter of 50 grains of dry ifinglafs, produced from the infufion a precipitate that weighed nearly 91 grains; and, in another inftance, a folution containing 30 grains of ifinglafs, gave about 56 grains; fo that, taking the mean of the two experiments, and allowing for the fmall quantity of infoluble matter in ifinglafs, we may conclude, that 100 grains of the compound of gelatine and tannin, formed by precipitation from faturated folutions, contain about 54 grains of gelatine, and 46 of tannin.

Animal matters and the infufion of galls.

Precipitate by gelatine contains 54 gelatine and 46 tanin nearly.

A piece of dry calf-fkin, perfectly free from extraneous matter, that weighed 180 grains, after being prepared for tanning by long immerfion in water, was tanned in a portion of the infufion, being expofed to it for three weeks. When dry, the leather weighed 295 grains: fo that, confidering this experiment as accurate, leather quickly tanned by means of an infufion of galls, confifts of about 61 grains of fkin, and 39 of vegetable matter, in 100 grains.

A fkin quickly tanned contains about three parts fkin and one vegetable matter.

After depriving a portion of the infufion of all its tanning matter, by repeatedly expofing it to the action of pieces of fkin, I found that it gave a much flighter colour to oxigenated

The exhaufted infufion contained lefs gallic acid; probably from decompofition by expofure.

fulphate

ſulphate of iron, than an equal portion of a ſimilar infuſion which had been immediately precipitated by ſolution of iſinglaſs; but I am inclined to attribute this effect, not to any abſorption of gallic acid by the ſkin, but rather to the decompoſition of it by the long continued action of the atmoſphere; for much inſoluble matter had been precipitated, during the proceſs of tanning, and the reſidium contained a ſmall portion of acetous acid.

Component parts of Aleppo galls. In aſcertaining the quantity of tannin in galls, I found that 500 grains of good Aleppo galls gave, by lixiviation with pure water till their ſoluble parts were taken up, and ſubſequent ſlow evaporation, 185 grains of ſolid matter. And this matter, examined by analyſis, appeared to conſiſt,

Solid matter.

Of tannin - - - - - - -	130 grains.
Of mucilage, and matter rendered inſoluble by evaporation - - - - - -	12
Of gallic acid, with a little extractive matter	31
Remainder, calcareous earth and ſaline matter	12

Reſidual fluid. The fluid obtained by the laſt lixiviation of galls, as M. Deyeux obſerved, is pale green; and I am inclined to believe, that it is chiefly a weak ſolution of gallate of lime. The aſhes of galls, deprived of ſoluble matter, furniſh a very conſiderable quantity of calcareous earth. And the property which M. Deyeux diſcovered in the liquor of the laſt lixiviations, of becoming red by the action of acids, and of regaining the green colour by means of alkalis, I have obſerved, more or leſs, in all the ſoluble compounds containing gallic acid and the alkaline earths.

III. EXPERIMENTS AND OBSERVATIONS ON CATECHU OR TERRA JAPONICA.

Catechu or terra japonica. The extract called catechu is ſaid to be obtained from the wood of a ſpecies of the Mimoſa *, which is found abundantly in India, by decoction and ſubſequent evaporation.

Hiſtory and external characters from Bombay and Bengal. There are two kinds of this extract; one is ſent from Bombay, the other from Bengal; and they differ from each other more in their external appearance than in their chemical compoſition. The extract from Bombay is of an uniform texture, and of a red-brown tint, its ſpecific gravity being generally

* See Kerr. Medical Obſervations, Vol. V. p. 155.

about

about 1.39. The extract from Bengal is more friable, and less consistent; its colour is like that of chocolate externally, but, when broken, its fracture presents streaks of chocolate and of red-brown. Its specific gravity is about 1.28. Their tastes are precisely similar, being astringent, but leaving in the mouth a sensation of sweetness. They do not deliquesce, or apparently change, by exposure to the air.

The discovery of the tanning powers of catechu, is owing to the President of the Royal Society, who, concluding from its sensible properties that it contained tannin, furnished me, in December, 1801, with a quantity for chemical examination. Sir J. Banks first concluded that it contains tanin.

In my first experiments, I found that the solutions of catechu copiously precipitated gelatine, and speedily tanned skin; and, in consequence, I began a particular investigation of their properties. Experiments shew the truth of this conclusion.

The strongest infusions and decoctions of the two different kinds of catechu, do not sensibly differ in their nature, or in their composition. Their colour is deep red-brown, and they communicate this tinge to paper; they slightly redden litmus-paper; their taste is highly astringent, and they have no perceptible smell. Both kinds agree in their nature, &c.

The strongest infusions that I could obtain from the two kinds of catechu, at 48° Fahrenheit, were of the same specific gravity, 1.057. But, by long decoction, I procured solutions of 1.102, which gave, by evaporation, more than $\frac{1}{8}$ of their weight of solid matter. The strongest infusions of catechu

Five hundred grains of the strongest infusion of catechu from Bombay, furnished only 41 grains of solid matter; which, from analysis, appeared to consist of 34 grains of tannin, or matter precipitable by gelatine, and 7 grains that were chiefly a peculiar extractive matter, the properties of which will be hereafter described. The quantity of solid matter given by the strongest infusion of the Bengal catechu, was the same, and there was no sensible difference in its composition. Portions of these solid matters, when incinerated, left a residuum which seemed to be calcareous; but it was too small in quantity to be accurately examined, and it could not have amounted to more than $\frac{1}{200}$ of their original weights. afforded 41 solid matter; viz. 34 grains tanin and 7 peculiar extract.

The strongest infusions of catechu acted upon the acids and pure alkalis in a manner analagous to the infusion of galls. With the concentrated sulphuric and muriatic acids, they gave dense The infusion examined by acids and alkalis;

denſe light fawn coloured precipitates. With ſtrong nitrous acid they effervefced; and loſt their power of precipitating the ſolutions of iſinglaſs, and the ſalts of iron. The pure alkalis entered into union with their tannin, ſo as to prevent it from being acted upon by gelatine.

— with the ſoluble earths.

When the ſolutions of lime, of ſtrontia, or of barytes, were poured into the infuſions, copious precipitates, of a ſhade of light brown, were formed; and the reſidual fluid aſſumed a paler tint of red, and was found to have loſt its power of precipitating gelatine.

Lime, magneſia, carbonate of magneſia.

After lime had been boiled for ſome time with a portion of the infuſion, it aſſumed a dull red colour. The liquor that paſſed from it through the filtre had only a faint tint of red, did not act upon gelatine, and ſeemed to contain only a very ſmall portion of vegetable matter. Pure magneſia, when heated with the infuſion, acted upon it in an analogous manner; the magneſia became light red, and the reſidual fluid had only a very ſlight tinge of that colour. With carbonate of magneſia, the infuſion became deeper in colour, and loſt its power of precipitating gelatine; though it ſtill gave, with oxygenated ſulphate of iron, a light olive precipitate.

Carbonates of potaſh, ſoda, ammonia.

The carbonates of potaſh, of ſoda, and of ammonia, in their concentrated ſolutions, produced only a ſlight degree of turbidneſs in the infuſion of catechu; they communicated to them a darker colour, and deprived them of the power of acting upon gelatine; though this power was reſtored by the addition of an acid.

Expoſure to the air.

After the mixture of the ſolution of carbonate of potaſh and the infuſions had been expoſed to the atmoſphere for ſome hours, a brown cruſt was found to have formed upon its ſurface, and a ſlight precipitation had taken place

Aluminous ſalts on infuſion of catechu.

The ſalts of alumine precipitated the infuſions, but leſs copiouſly than they precipitate the infuſion of galls. A ſimilar effect was produced by nitrate of potaſh, ſulphate of magneſia, pruſſiate of potaſh, and many other neutral ſalts.

Nitrate or acetite of lead.

The nitrate, or acetite, of lead, in concentrated ſolution, when poured into the infuſion, produced in it a denſe light brown precipitate, which gave to the fluid a gelatinous appearance. After this effect, there was no free acid found in it; and both the tannin and the extractive matter ſeemed to have been carried down, in union with a portion of the metallic ſalt.

The

The solution of muriate of tin, acted upon the infusion of catechu in a manner similar to that in which it acts upon the infusion of galls.

Muriate of tin.

The least oxygenated sulphate of iron produced no change in the infusion. With the most oxygenated sulphate it gave a dense black precipitate, which, when diffused upon paper, appeared rather more inclined to olive than the precipitate from galls.

Sulphates of iron.

The infusions were precipitated by the solution of albumen.

The precipitates by gelatine had all a pale tint of red-brown, which became deeper when they were exposed to the air. The compound of gelatine and the tannin of the strongest infusions of catechu appeared, by estimation of the quantity of isinglass in the solutions used for their precipitation, to consist of about 41 parts of tannin, and 59 of gelatine.

Precipitates from infusion of catechu by gelatine 41 tannin and 59 gelatine.

Of two pieces of calf-skin which weighed, when dry, 132 grains each, and which had been prepared for tanning, one was immersed in a large quantity of the infusion of catechu from Bengal, and the other in an equal portion of the infusion of that from Bombay. In less than a month they were found converted into leather. When freed from moisture, by long exposure in the sunshine, they were weighed. The first piece had gained about 34 grains; and the second piece $35\frac{1}{2}$ grains. The leather was of a much deeper colour than that tanned with galls, and on the upper surface was red-brown. It was not acted on by hot or cold water; and its apparent strength was the same as that of similar leather tanned in the usual manner.

Calf skin (quickly) tanned by catechu afforded good leather.

The increase of weight was rather less than one fifth of the leather.

In examining the remainder of the infusions of catechu, in which skin had been converted into leather, I found in them much less extractive matter than I had reason to expect, from the comparative analysis of equal portions of the unaltered infusions made by solutions of gelatine. At first, I was inclined to suppose that the deficiency arose from the action of the atmosphere upon the extractive matter, by which a part of it was rendered insoluble. But, on considering that there had been very little precipitation in the process, I was led to adopt the supposition, that it had entered into union with the skin, at the same time with the tannin; and this supposition was confirmed by new experiments.

Little extractive matter left.

It had in part combined with the skin.

Both kinds of catechu are almost wholly soluble in large quantities of water; and, to form a complete solution, about 18 ounces

Water dissolves nearly the whole of catechu.

ounces of water, at 52°, are required to 100 grains of extract. The residuum seldom amounts to $\frac{1}{14}$ of the original weight of the catechu ; and, in most cases, it is found to consist chiefly of calcareous and aluminous earths, and of fine sand, which, by accident or design, had probably been mixed with the primitive infusion at the time of its evaporation.

Alcohol dissolves much ; and leaves mucilage.

A considerable portion of both kinds of catechu is soluble in alcohol ; but, after the action of alcohol upon it, a substance remains of a gelatinous appearance and a light brown colour, which is soluble in water, and is analogous in its properties to gum or mucilage.

The peculiar extract of catechu is much less soluble than tannin in water.

The peculiar extractive matter in the catechu, is much less soluble in water than the tanning principle ; and, when a small quantity of water is used to a large quantity of catechu, the quantity of tannin taken up, as appears from the nature of the strongest infusion, is very much greater than that of the extractive matter.

And is more soluble by heat.

The extractive matter is much more soluble in warm water than in cold water ; and when saturated solutions of catechu are made in boiling water, a considerable quantity of extractive matter, in its pure state, falls down, as the liquor becomes cool.

Repeated lixiviations of catechu in water, leave the peculiar extract.

The peculiar extractive matter of the catechu may be likewise obtained, by repeatedly lixiviating the catechu, when in fine powder, till the fluids obtained cease to precipitate gelatine ; the residual solid will then be found to be the substance in question.

Its sensible qualities.

The pure extractive matter, whether procured from the Bombay or Bengal catechu, is pale, with a faint tinge of red-brown. It has no perceptible smell ; its taste is slightly astringent ; but it leaves in the mouth, for some time, a sensation of sweetness, stronger that given by the catechu itself.

Solution in water and in alcohol.

Its solution in water is at first yellow-brown ; but it gains a tint of red by exposure to the air. Its solution in alcohol does not materially change colour in the atmosphere ; and it is of an uniform dull brown.

Habitude with tests ;

The extractive matter, whether solid or in solution, was not found to produce any change of colour upon vegetable blues.

— with alkalis.

It became of a brighter colour by the action of the alkalis ; but it was not precipitated from its solution in water by these bodies, nor by the alkaline earths.

The aqueous ſolution of it, when mixed with ſolutions of nitrate of alumine and of muriate of tin, became ſlightly turbid. — with other bodies.

To nitrate of lead, it gave a denſe light brown precipitate.

It was not perceptibly acted upon by ſolution of gelatine; but, when ſolution of ſulphate of alumine was added to the mixture of the two fluids, a conſiderable quantity of ſolid matter, of a light brown colour, was immediately depoſited.

To the ſolution of oxygenated ſulphate of iron, it communicated a fine graſs-green tint; and green precipitate was depoſited, which became black by expoſure to the air.

It was not precipitated by the mineral acids.

Linen, by being boiled in the ſtrongeſt ſolution of the extractive matter, acquired a light red-brown tint. The liquor became almoſt colourleſs; and, after this, produced very little change in the ſolution of oxygenated ſulphate of iron. Linen abſtracts its tinge.

Raw ſkin, prepared for tanning by being immerſed in the ſtrong ſolution, ſoon acquired the ſame kind of tint as the linen. It united itſelf to a part of the extractive matter; but it was not rendered by it inſoluble in boiling water. — as does raw ſkin.

The ſolid extractive matter, when expoſed to heat, ſoftened, and became darker in its colour, but did not enter into fuſion. At a temperature below that of ignition, it was decompounded. The volatile products of its decompoſition were, carbonic acid, hydrocarbonate, and water holding in ſolution acetous acid and a little unaltered extractive matter. There remained a light and very porous charcoal. The ſolid extract ſoftens but does not fuſe by heat. Deſtructive decompoſition.

In conſidering the manner in which the catechu is prepared, it would be reaſonable to conclude, that different ſpecimens of that ſubſtance muſt differ in ſome meaſure in their compoſition, even in their pure ſtates; and, for the purpoſes of commerce, they are often adulterated to a conſiderable extent, with ſand and earthy matter *.

In attempting to eſtimate the compoſition of the pureſt catechu, I ſelected pieces from different ſpecimens, with which I was ſupplied by the preſident, and reduced them together into powder; mixing, however, only thoſe pieces which were from catechu of the ſame kind. Component parts of catechu.

* One ſpecimen that I examined, of the terra japonica of commerce, furniſhed, by incineration, $\frac{1}{7}$ of ſand and earthy matter; and another ſpecimen, nearly $\frac{1}{6}$.

Two hundred grains of the powder procured in this way, from the catechu of Bombay, afforded by analyſis.

Bombay catechu.

	Grains.
Tannin - - - - - - - - -	109
Peculiar extractive matter - - - - - -	68
Mucilage - - - - - - - - -	13
Reſidual matter; chiefly ſand and calcareous earth	10

The powder of the Bengal catechu gave, by ſimilar methods of analyſis, in 200 grains,

Bengal catechu.

	Grains.
Tannin - - - - - - - - -	97
Peculiar extractive matter - - - - -	73
Mucilage - - - - - - - - -	16
Reſidual matter; ſand, with a ſmall quantity of calcareous and aluminous earths - - - - -	14

In the ſolid ſamples the tannin and mucilage are imperfectly mingled.

In examining thoſe parts of the catechu from Bengal which were differently coloured, I found the largeſt proportion of tannin in the darkeſt part of the ſubſtance; and moſt extractive matter in the lighteſt part. It is probable that the inequality of compoſition in this catechu, is owing to its being evaporated and formed without much agitation; in conſequence of which, the conſtituent parts of it that are leaſt ſoluble, being firſt precipitated, appear in ſome meaſure diſtinct from the more ſoluble parts, which aſſume the ſolid form at a later period of the proceſs.

Pale catechu moſt in requeſt. It contains more of the extract.

From the obſervations of Mr. Kerr *, it would appear, that the pale catechu is that moſt ſought after in India; and it is evidently that which contains moſt extractive matter. The extractive matter ſeems to be the ſubſtance that gives to the catechu the peculiar ſweetneſs of taſte which follows the impreſſion of aſtringency; and it is probably this ſweetneſs of taſte which renders it ſo agreeable to the Hindoos, for the purpoſe of chewing with the betle-nut.

(To be continued.)

* Medical Obſervations, Vol. V. page 155.

X.

Memoir on the Nature and the new Properties of the Pruſſic Acid. By CIT. CURAUDAU, *correſponding Member of La Société libre des Pharmaciens de Paris* *.

Uncertainties ſtill exiſting as to the theory of Pruſſic acid.

THE Pruſſic colouring matter has been long known in chemiſtry, but we have yet no correct knowledge of its nature and different modifications; chemiſts are not even agreed as to the formation of this ſingular ſubſtance. Some admit that oxigen forms a part of its compoſition, and others aſſert it to be an acid without oxigen. Indeed this compoſition performs the functions of an acid in many inſtances; but in certain others, and particularly in its ſimple ſtate, it ceaſes to exhibit the properties common to other acids. This diverſity of opinions on a ſubſtance which we can compoſe and decompoſe, reduce and combine at pleaſure, ſeems to contradict our chemical knowledge, and to carry us from that perfection towards which *perfect* ſcience ought continually to tend.

Whether an acid can exiſt without oxigen.

In fact, if in the claſs of acids it were proved that there were one without oxigen; might we not conclude that the baſes might, as well as oxigen, contain the acidifying principle, and would it not follow that we have gratuitouſly aſcribed to vital air the property of generating acid?

The preſent reſearch is to determine the cauſes by which Pruſſic colouring matter is acidified, and the contrary.

It is of importance then, under theſe circumſtances, to fix the opinions of the chemiſts on this point, and to determine what may be the concurring cauſes that give the pruſſic colouring matter the characteriſtic properties of acids, or to ſeparate it from their claſs. This is the problem which I have undertaken to reſolve. If I have not fulfilled the taſk, I dare at leaſt flatter myſelf that my obſervations will prove uſeful to the progreſs of ſcience, and that they may deſerve the attention of the chemical world.

Adjuſtment of terms.

In order to avoid the continual repetition of Pruſſic acid, Pruſſic colouring matter, &c. I ſhall in future deſcribe this acid under the name of "*Pruſſire,*" its combinations without oxigen "*Pruſſiurets,*" and its oxigenated combinations "*Pruſſiates.*" This correction of Nomenclature becomes neceſſary not

* From Les Annales de Chimie, No. 137. Vol. XLVI. p. 148.

not only to ſhorten the expreſſions, but to explain in proper terms ſuch notions as indicate the real ſtate of the ſubſtance under deſcription.

The gaſeous pruſſic acid has leſs oxigen than when in combination.

Chemiſts have never diſtinguiſhed the Pruſſic acid, ſaturating a metallic ſubſtance from the gazeous *Pruſſire*. They have always believed on the contrary that this laſt, extracted from any Pruſſiate whatever, was the ſame as in its ſtate of combination. But on a nearer examination, we find it impoſſible to obtain ſimple Pruſſic acid; that it can only exiſt in a combined ſtate; that its combinations can only take place by virtue of the oxigen afforded by the baſe to which it is united: laſtly, that the ſimple pruſſire cannot exiſt with the preſence of oxigen without an immediate deſtruction of the affinity which held its three component principles together. The carbon in this radical is particularly diſpoſed to unite with oxigen, and at the inſtant when the ſubtraction of this principle is made, the azote and the hydrogen become liberated, and immediately form ammonia, which is alſo ſaturated by the carbonic acid at the moment of their mutual production. Water is the oxigenated compound which has the ſtrongeſt effect in producing the decompoſition of pruſſire, without ſeparating its elements; that is to ſay, the reſult of this decompoſition is always carbonic acid and ammonia.

It cannot exiſt with more oxigen: For this laſt will acidify its carbon, while the azote and hydrogen form ammonia; and hence carbonate of ammonia will be had.

Suppoſed incongruity of the volatility (or gazeous form) of pruſſire, and its original fixity.

The fixity of pruſſire ſhewn by the high temperature at which it is formed, does not appear to me to agree with what is already known relative to its extreme volatility, and of its little diſpoſition to unite directly with ſaline baſes. I thought it neceſſary to direct my enquiries to this object, in order to aſcertain, if poſſible, the cauſe which produced ſuch oppoſite properties in the ſame ſubſtance, and which, as I apprehend, muſt ariſe from facts not yet known and underſtood.

The fixity of pruſſire, as ſhewn by the great heat it ſupports during calcination does not ariſe from oxigen.

I could not attribute the fixity of pruſſire to the oxigen, ſince, by the experiments which I publiſhed five years ago *, I have ſhewn that this principle always oppoſes its formation, and even deſtroys it when already formed. Thus, from the moment it was proved that the calcinations intended to form the pruſſire is ineffectual wherever oxigen is preſent, I could no longer admit that this compound, when formed by calcination, with an alkali, can derive its fixity from oxigen. It became

* Journal de Pharmacie, cahier de Germinal, An 6.

came neceffary, therefore, for me to direct my enquiries according to the indications afforded by my own experiments, and to continue them till I fhould become acquainted with the caufe of fixity in a fubftance which immediately on its formation, poffeffed properties quite oppofite to thofe which it feemed to poffefs during the action of calcination.

Perfeverance in varying my enquiries on this object, gave me an opportunity of making many important obfervations, and to afcertain, by decifive experiments, that the Pruffic calcination gives nothing more than the carbonated azote of potafh, and that the hydrogen which enters into the compofition of the pruffire, is always the refult of a combination which takes place fubfequent to the calcination. Hence I faw that it was this circumftance which fo long produced the chemical illufion, and from which we ftill confound this production with that contained in the crucible. But it is a phænomenon worthy of remark, and of which the explanation is very proper to throw light on the nature and formation of pruffire, that the carbonated azote of potafh poffeffes the property of inftantly decompofing water; and at the moment the water is decompofed, the pruffire is formed, together with ammonia and carbonic acid. Though this immediate production of three new compounds is the refult of very complicated affinities, which are difficult to be well made out; we may neverthelefs fufpect that the potafh, which is always found in excefs in pruffic calcinations, is the fubftance which in this cafe preponderates in the affinities, and that this fubftance, by a predifpofing attraction, promotes the oxigenation of the carbon at the expence of the oxigen of the water; the hidrogen, which being fet at liberty by this fubtraction of oxigen, unites at the fame time with the carbonated azote of potafh. One part of the hidrogen is thus employed in forming ammonia, while the other ferves to form the pruffire, which was not pruffire for want of the hydrogen, its third element. It is ftill farther in favour of this theory that the potafh in its progrefs to faturation with carbonic acid may, by long ebullition, fucceffively convert the greater part of the pruffire, into ammonia and carbonic acid; this decompofition ufually continues till the potafh is completely faturated with the carbonic acid. Hence it is that a very good pruffic lixivium may be partly decompofed by fimple ebullition, and that in all pruffic calcinations, thofe which

The calcination affords only carbonated azote of potafh. The hidrogen is not given till water is applied.

— which fluid is inftantly decompofed.

Explanation of the effect of the affinities.

The pruffic lixivium may be decompofed by water and lofs incurred.

which are moſt ſtrongly heated always form lixivium ſaturated with the carbonic acid ; this can only happen at the expence of the oxigen of the water and the carbon of the carbonated azote of potaſh : The potaſh, therefore, as I have already obſerved, contributes in part, by a prediſpoſing attraction, to the ſucceſſive developement of all theſe new combinations.

After having proved that carbonated azote of potaſh inſtantly decompoſes water, and that water alſo ſucceſſively decompoſes the pruſſire; which is always by a diminution of its quantity ; I ſhall now ſhew the means which muſt be uſed to prevent the deſtruction of this ſubſtance, and ſhew that they perfectly agree with the theory I have here laid down.

Remedy againſt the loſs by uſing water with the pruſſic calcined matter.

The deſtructibility of pruſſire of potaſh being highly prejudicial in large operations, I directed my reſearches to the means of preventing that loſs. The reſult of my obſervations is, that, in order to ſtop this decompoſition, it is an indiſpenſable condition, never to wet a pruſſic calcination but with a ſolution of ſulphate of iron at the maximum of oxigenation ; this method is intitled to the moſt decided preference ; for in this caſe the pruſſire, which is formed at the moment of the mixture, being in a ſtate favourable to the ſaturation of potaſh, it becomes fixed at the expence of the oxigen of the oxide of iron, and in this ſtate it is pruſſic acid. I muſt obſerve that this oxigenated combination of pruſſire cannot take place but by the concurrence of metallic ſubſtances already oxigenated ; and that it is only then that an equilibrium of affinities is eſtabliſhed between the potaſh, the pruſſire, and the metallic oxide ; which produces a complex and energetive combination, that I propoſe to call pruſſiate of potaſh by the oxide of iron ; which method of nomenclature I propoſe to apply to each ſeveral metallic oxide which enters into the compoſition of pruſſiate of potaſh. All theſe combinations cryſtallize eaſily by evaporation, and by cooling the lixivium we may obtain very regularly formed cryſtals.

Sulphate of iron uſed inſtead.

The pruſſire can only be oxigenated by a metallic oxide.

Inference.

The conſequences we ſhould naturally draw from theſe latter obſervations ſhew therefore irrevocably that the pruſſire owes its acid or neutraliſing property to the oxigen of the oxide of iron, and that as the quantity of water decompoſed in this caſe does not exceed what is neceſſary to ſupply the hydrogen in the pruſſire, we muſt accordingly obtain more abundant products than in any other caſe.

I may

I may obferve, in fupport of what I have here advanced, that M. Berthollet, whofe fagacity is well known, has proved, more than fifteen years ago, by a very accurate analyfis, that what was called the pruffic acid, does not contain oxigen. It is true indeed, that there are many chemifts no lefs celebrated, who have not coincided in this opinion, and have confidered what M. Berthollet had advanced as very far from complete proof. I muft, neverthelefs, infift that the labours which that learned chemift has made known on this object, are a prelude, in fome fort, to the difcoveries which remained to be made, in order to complete the hiftory of the properties of the pruffic acid.

Berthollet firft afferted that pruffic gas contains no oxigen.

It remains then very evidently demonftrated from thefe refults, as well as thofe obtained by M. Berthollet, how far this theory is fatisfactory, and how much it enables us to explain facts, refpecting which we have hitherto had no very correct notions. Laftly, from all thefe new applications, we may, with a knowledge of the caufes, fabricate in the large way, cryftals of pruffiate of potafh, by oxide of iron. It is now a confiderable time fince I firft prepared fimilar cryftals, but without being able to eftablifh a theory fufficiently clear to be communicated.

The theory is fatisfactory and the refults beneficial.

It is to be remarked that Pruffian blue obtained with the pruffiate of potafh by the oxide of iron, gives a blue of the moft exquifite beauty. Its interior fracture is fmooth and of a copperifh hue, fimilar to that feen in indigo of the beft quality. An obfervation which equally deferves mention is, that the pruffiate of potafh by oxide of iron has the property of forming with a hot folution of fulphate of alumine, a magma analogous to that of boiled fecula. If the mixture be diluted with water to favour the feparation of the precipitate it is then of a fky blue colour. This precipitate wafhed and dried becomes of a deep blue, approaching to black; its fracture is fmooth and has a refinous appearance; it is the pruffiate of alumine by the oxide of iron.

The Pruffian blue greatly improved.

Pruffiate of alumine.

Recapitulation.

It refults from all that has been here faid:

Recapitulation.

1. That the pruffic calcination contains but two of the elements of pruffire, azote and carbon; and that the third element neceffary to the compofition of the acid, is the refult of a combination formed after the calcination.

1. Pruffic calcination gives only azote and carbon along with the alkali.

2. That

2. which is a new compound,

2. That the carbonated azote of potafh is a new compound in chemiftry, from our acquaintance with which we may fucceed in decompofitions which have hitherto been attempted without fuccefs.

3. capable of inftantly decompofing water.

3. That the property which carbonated azote of potafh has of inftantly decompofing water produces the pruffic acid, and that it is the potafh particularly which affifts the oxigination of the carbon and the combination of the hydrogen with the azote and the carbon.

4. propofed nomenclature of its compounds,

4. That we muft now call the fuppofed pruffic acid by the name of pruffire, becaufe its properties are analogous to thofe of all the radicals. Its combinations without oxigen will be called pruffiurets, and thofe which are oxiginated, pruffiates; with refpect to the latter, when they are not directly combined with a metallic fubftance, it will be neceffary to mention along with the direct bafe, the metallic oxide which may have given them the properties common to the acids. For example, the combination of pruffire with potafh will be called pruffiuret of potafh; with oxide of iron, pruffiate of iron, and with potafh and oxide of iron, pruffiate of potafh by oxide of iron. With refpect to thofe metallic pruffiates which are capable of paffing fucceffively into different degrees of oxigenation they may be defcribed by their colour, which will fufficiently indicate the ftate of the pruffiate.

5. water decompofes pruffiuret and occafions lofs,

5. That the continued action of water upon the pruffiuret of potafh is able to change it completely into carbonic acid and ammonia. This muft fhew that the hydrogen in the pruffire is not combined with the azote in the proportion neceffary to the compofition of ammonia, and that it muft be by the lofs of hydrogen in the water that the pruffire paffes fucceffively into the ammonial ftate.

6. Which may be prevented by ufing fulphate of iron,

6. That the decompofition of part of the pruffire may be prevented, by never moiftening a pruffic calcination, but with a folution of fulphate of iron at the maximum of oxigination.

7. the pruffic acid gas is a mere radical, which acquires its acid properties by oxigen from an oxide.

7. Laftly: What is called pruffic acid gas, is not an acid, but only the pruffic radical, and that it acquires the acid or neutralizing property only by means of the oxigen which is imparted to it by a metallic oxide, of which the concurrence is neceffary to form with the faline bafis a ftrong and durable compound.

Defcrip-

XI.

Defcription of the Subterraneous Graphometer invented by M. Komarzewski, *F. R. S. and prefented to the National Inftiftitute of France* *.

THIS inftrument is a fimple conftruction of the common theodilite, and is calculated to anfwer the author's intention in operations like thofe of mining, where great accuracy is not required. Defcription of a fubterraneous graphometer.

The roughest kind of furveys are made by taking angles with the compafs, and thefe are ftill ufed in mines, and in the diftricts of unfettled countries. Our author has taken the next procefs, and obtains confiderably more accuracy, without deviating from fimplicity and cheapnefs.

This fubterraneous graphometer is compofed of three principal parts, *viz.* a circular horizontal plate, a circular vertical plate, and a pedeftal, which are refpectively reprefented in perfpective, (Figs. 2, 3 and 5, Plate XV.)

The horizontal Plate A. Fig. 2. is divided into 360° doubly numbered, the fubdivifions are carried to 15′, this is confidered fufficient for mines where there is feldom light enough to obferve a more minute divifion: for operations on the furface of the earth a Vernier's fcale may be added.

Within the line of divifions above mentioned, is that of the magnetic compafs: which gives the reduction of hours into degrees, and that of degrees into hours.

This plate meafures the horizontal angles which the miners call the *direction.*

Through its centre paffes the cylinder *a*, on which turns the hollow cylinder, which forms the centre of the horizontal index *bb*, and carries the vertical plate B, as reprefented in Fig. 1.

The horizontal index is one inch in breadth, and is increafed at the centre by two circular fegments *c c* caft with it, in order to give the vertical plate a more firm bearing on the horizontal plate at the top of the cylinder *a*, a fcrew *a* is inferted to confine the cylinder.

* From a folio publication in Englifh, entitled, Memoir on a Subterraneous Graphometer, &c. publifhed at Paris by Pougens, 1803.

The

Defcription of a fubterraneous graphometer.

The hook attached to one extremity of the horizontal index is intended for the fufpenfion of a plumb line.

The vertical plate B, Fig. 3, is only fix inches in diameter, in order that the extremities of the horizontal index may be left perfectly clear.

Two indexes *d d*, are moveable round its centre *c*, which meafure the angles of inclination. They are fomewhat narrower than the horizontal index. Their extremities are likewife furnifhed with threads to mark the quantity of the angles to the extremities of thefe indexes, and in the direction of their axes are fixed two hooks in order to receive the cords which put the indexes in motion.

The vertical plate is divided on each fide by two lines paffing through its centre, one of which is horizontal and the other vertical, the one mark the diameter, and the other the radius. The upper part of the plate is likewife divided on each fide into twice 90. This divifion commences at the horizontal diameter, and proceeds to the vertical radius, the lower part which is but a fegment of the circle, contains only on each fide 30°, this is fufficient for a lode or gallery in mines, more confiderable inclinations being meafured like the depth of a fhaft.

Thefe two plates with their indexes form the graphometer, the other parts belong to the pedeftal, on which it may be moved as occafion requires. Its dimenfions depend upon the fpace in which the graphometer is to be employed. The pedeftal is reprefented at Fig. 5, as follows,

A board EE about 18 inches long, fix inches wide, and one in thicknefs, receives in a focket made its centre, the brafs ball K, furmounted by the cylinder H caft with the ball, the board is bored towards its extremities, to receive brafs fcrews, by which it is fixed on fome wooden bar in the mines.

The ball K is covered by a fquare plate *g*, which is concave and perforated in its centre, and is fixed to the board E by the four fcrews *h*: it is by loofening or tightening them that the graphometer is fixed or levelled.

On the cylinder H turns the hollow cylinder F, which is caft with the fmall plate D, this hollow cylinder is furnifhed with a tightening fcrew G, to fix the graphometer when placed upon the plate D: the graphometer is fixed to this plate by two ftaples

ſtaples *ff* ſoldered to the horizontal plate A, which enter two correſponding holes in the plate D, and are confined by the bolts *ff* ſliding in grooves ſoldered beneath the ſame plate.

Deſcription of a ſubterraneous graphometer.

In the centre of the plate D as well as in the centre of the inferior part of the ball K, is a female ſcrew *i*, to receive the ſolid cylinder L, Fig. 4, the uſe of which will be ſeen in the ſolution of the problems. This cylinder is furniſhed with a moveable collar *m*, having a hook in its centre, to which the other extremity of the cord may be attached.

Fig. 1. is a perſpective view of the graphometer, in which A is the horizontal plate, B the vertical plate, *a a* the centre of the vertical plate, &c. *b b* the horizontal index, *d d* the vertical indexes.

From this diſpoſition it reſults that, when one direction is known, which for the firſt operation may be determined by the magnetic compaſs, or in mining ſtill better by a meridian line paſſing through one of the principal ſhafts, the inſtrument may with facility be directed to any given point, and all plans be executed which are neceſſary in operations of mining, without making uſe of the magnetic needle, and that the directions and inclinations may be taken at the ſame time, ſince in all theſe operations it is only neceſſary to obſerve the angles indicated by the inſtrument, to meaſure the length of the line, and to calculate right angled triangles, of which three parts are known.

The pedeſtal is intended to be placed at the point of the angle ſought, and the cords ſtretched to it from the graphometer, the cord is fixed to the hook of the graphometer by a noose, and its other extremity to the cylinder L of the pedeſtal, by means of the ſcale and pin repreſented at Fig. 6.

XII.

Experiments and Obſervations on the various Alloys, on the Specific Gravity, and on the comparative Wear of Gold. Abſtracted from the Memoir of CHARLES HATCHETT, ESQ. F. R. S*.

INTRODUCTION.

Appointment of H. Cavendiſh Eſq. and C. Hatchett Eſq. to make experiments on the wear of gold;

THE Lords of the Committee of his Majeſty's moſt honourable Privy Council, appointed by his Majeſty, on the 10th of February, 1798, to take into conſideration the ſtate of the coins of this kingdom, having among other circumſtances remarked the conſiderable loſs which the gold coin appeared to have ſuſtained by wear within certain periods, and being deſirous to aſcertain whether this loſs was occaſioned by any defect, either in the quality of the ſtandard gold or in the figure or impreſſion of the coins, were pleaſed to requeſt that Henry Cavendiſh, Eſq. F. R. S. and the author of the preſent memoir would examine, by ſuch experiments as ſhould be deemed requiſite, whether any of theſe defects really exiſted.

Two queſtions were to be principally decided,

different in compoſition;

1ſt. Whether very ſoft and ductile gold, or gold made as hard as is compatible with the proceſs of coining, ſuffers the moſt by wear, under the various circumſtances of friction to which coin is ſubjected in the courſe of circulation?

or in figure.

2dly. Whether coin with a flat, ſmooth, and broad ſurface, wears leſs than coin which has certain protuberant parts raiſed above the ground or general level of the pieces?

The reſearch is new.

The great value of the material, had hitherto prevented private individuals from aſcertaining theſe facts by experiment; and, as a public concern; this ſubject of inveſtigation, although ſo important to political economy and to ſcience, does not appear to have been noticed by any European government, until the Right Honourable and enlightened Members of the above-mentioned Committee propoſed the inquiry, and furniſhed the requiſite means for making the experiments †.

* In the Philoſophical Tranſactions for 1803.

† Theſe experiments were begun in the latter end of 1798, and were completed in April, 1801.

SECTION I.

SECTION I.

ON THE VARIOUS ALLOYS OF GOLD.

The wear of coin is fubject to be modified by certain phyfical properties, fuch as ductility and hardnefs, which vary in degree, according to the chemical effects produced by different metallic fubftances, when employed in certain proportions as alloys. From thefe confiderations, it appears proper, Plan of experiments.

Firft, to examine the effects which the various metals produce upon gold, when combined with it in given proportions, beginning with $\frac{1}{12}$, which is the ftandard proportion of alloy, and in certain cafes gradually decreafing to $\frac{1}{4}$ of a grain in the ounce of troy, or $\frac{1}{1920}$ part of the mafs. Effects of alloys.

Secondly, to examine the fpecific gravity of gold differently alloyed, and the caufes of certain variations to which it is liable. Specific gravities.

And, thirdly, to afcertain the effects of friction varioufly modified. Wear.

GOLD ALLOYED WITH ARSENIC.

Experiment I.

To 5307 grains of gold in fufion, 453 pure arfenic were added. Only fix grains remained in the ingot, which was brittle. Gold and arfenic.

Experiment II.

To 442 grs. gold were added 19 grs. copper, and on perfect fufion, 19 grs. pure arfenic, and the whole quickly poured out. None of the arfenic remained. Gold, copper, and arfenic.

Experiment III.

480 grains of gold in a fmall crucible was inclofed within two larger crucibles, well luted together. 480 grains of pure arfenic were included in the outer veffel. Strong and continued heat, *(a)* raifed fome arfenic in the white oxide, *(b)* and alfo a fmall portion of the gold, *(c)* and the gold was brittle, with a coarfe gray fracture, an increafe of 1,5 grains in weight. *(d)* It was difficult to expell the arfenic by heat. Gold and arfenic in clofe veffels.

Experiment IV.

A plate of gold was fufpended within two crucibles ground and luted together, pure arfenic being placed in the lower. By Gold expofed to the vapor of arfenic at a red heat.

By a full red heat, the arfenic rofe and combined with the furface of the plate, producing a very fufible compound, which ran down into the lower veffel. The plate, which had been $\frac{1}{30}$ of an inch thick, was become as thin as paper; but was ftill pure gold. The fufed compound was extremely brittle, and gray within. It contained a very large quantity of arfenic, but how much is not ftated.

Experiment V.

Variation of Exp. 4.

Variation of the preceding experiment. The arfenic was introduced through a hole after the ignition, and the heat not kept up. The furface of the plate was impregnated with arfenic; but the heat not being fufficient to caufe it to flow down, it adhered to the plate, and rendered it lefs flexible.

GOLD ALLOYED WITH ANTIMONY.

Experiment I. *to* VIII.

Gold and antimony.

Golds in various proportions and of different finenefs were combined with pure antimony by heat. Small proportions of the latter, produced a pale, dull, unmetallic colour, and rendered the compound exceedingly brittle. Minute quantities ftill impaired the metallic brilliancy, and caufed brittlenefs. Lefs than one quarter of a grain, in the ounce of gold, were fufficient to produce thefe effects. The vapors of antimony readily combine with gold in open as well as clofe veffels, and afford the fame changes.

GOLD ALLOYED WITH ZINC.

Experiment I. *to* VI.

Gold and zinc.

By thefe experiments, it was fhewn, that zinc is highly injurious to the ductility of gold; that a portion of it is eafily feparated from gold by heat; that, when a large quantity of gold is alloyed with the ftandard proportion of zinc, only part of the latter is fpeedily volatilized, but, when fmall quantities are treated, the whole of the zinc becomes feparated, and the gold remains pure; that, if zinc is previoufly combined with copper in the ftate of brafs, it is not fo eafily feparated by heat, when added to melted gold; and, laftly, that gold in fufion abforbs and retains a portion of zinc, when expofed to the latter metal in a volatilized ftate, even in open veffels.

GOLD

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GOLD ALLOYED WITH COBALT.

Experiment I. *to* IV.

This addition debafed the colour of the gold, and rendered it brittle. When to an alloy of about 10 parts gold, and 1 part copper, nearly half a quarter part of cobalt were added, the metal began to be ductile. Gold and cobalt.

GOLD ALLOYED WITH NICKEL.

Experiment I. *to* IV.

The colour and ductility of gold were injured by nickel, but lefs than by any other of the brittle metals. When to about eleven parts of gold, and one of copper, one fourth of a part of nickel was added, the compound was fcarcely brittle. Gold and nickel.

GOLD ALLOYED WITH MANGANESE.

Experiment I. *to* III.

The black oxide of manganefe ftrongly heated with gold did not affect it. But when olive oil had been previoufly burned on the oxide, this powder ftrongly and permanently heated with gold, rendered it pale and brittle. This alloy is lefs fufible than pure gold; it does not change by long expofure to the air; and the manganefe is defended from the ufual action of acids. It may be feparated by cupellation, or better by quartation and nitric acid. Gold and manganefe.

GOLD ALLOYED WITH BISMUTH.

Experiment I. *to* VI.

Bifmuth added to pure, or to alloyed gold, is exceedingly injurious to its colour and ductility. It produces thefe effects when the quantity is no more than $\frac{1}{2000}$ part of the mafs; and the combination takes place by the vapor of bifmuth, in open as well as clofe veffels. Gold and bifmuth.

GOLD ALLOYED WITH LE AD.

Experiment I. *to* IX.

Lead is nearly as injurious to gold as bifmuth; one quarter of a grain in the ounce producing complete brittlenefs. Its vapor in clofe veffels greatly contaminates gold, but in open veffels the effect was inconfiderable. Gold and lead.

GOLD ALLOYED WITH TIN.

Experiment I. *to* IV.

Gold and tin.

A confiderable portion, as $\frac{1}{10}$ of tin rendered gold paler and lefs ductile: fmall quantities as $\frac{1}{50}$ did not material injure it. Mr. Bingley, in confequence of Tillet's experiments, related in the Acad. Par. 1790, and in the Quarto Series of our Journal II. 140, 179, made experiments on an alloy of gold and tin, containing eight grains in the ounce of the latter metal. The facts are, 1. the earlier chemifts, probably as Mr. Hatchett obferves, mifled by a tin containing bifmuth, lead, antimony or zinc, did affirm that the fmalleft portion of that metal renders gold brittle; 2. Mr. Alchorne fhewed the fallacy of that general pofition, by experiments on fuch alloys. 3. Mr. Tillet fhewed that tho' gold alloyed with tin, may be hammered and laminated cold, yet it falls to pieces by the annealing heat, or cherry red; and laftly, Mr. Bingley finds that an annealing by five degrees of Wedgwood's pyrometer, or red vifible by day light, does not render the gold unfit for working; but that the cherry red or ten degrees blifters the furface, warps the bar, and caufes it to fall in pieces, by the fufion of the moft fufible metal; a general property on which the procefs of eliquation depends.

GOLD ALLOYED WITH IRON.

Experiment. I. *to* IV.

Gold and iron.

Gold, with and without copper, was alloyed with about $\frac{1}{11}$ part of iron, and in other experiments of caft iron, caft fteel, and iron wire, gave a metal of a pale yellowifh gray, approaching to a dull white, which was very ductile and laminable, and was ftamped with great eafe without annealing; though it was harder than gold.

EXPERIMENTS ON EMERY AND GOLD.

Emery and gold.

Gold was heated ftrongly, and for a long time, with emery in fine powder, and in fome trials previoufly heated with oil. No change was produced.

GOLD ALLOYED WITH PLATINA.

Gold and platina.

Fine gold, and alfo ftandard gold, were alloyed with platina. The compound was yellowifh-white, and very ductile.

 It

It was not thought neceſſary to extend the experiments, becauſe theſe metals have been much examined by other chemiſts *.

GOLD ALLOYED WITH COPPER.

From many experiments it appears, that the varieties of copper, in commerce, although ſimilar in aſpect, and other obvious properties, are far from being uniform in quality; ſo that many of them are by no means ſufficiently pure to be employed as an alloy for gold; but render it brittle.

Gold and copper. Many kinds of copper render gold brittle.

And the different effects produced by the moulds of iron and thoſe of ſand, are ſuch as fully prove, that copper which is not perfectly pure, and which has a tendency to render gold brittle, acts more powerfully, in this reſpect, when the alloyed maſs is caſt in ſand than when it is caſt in iron; and, all things being conſidered, there is reaſon to conclude, that moulds of iron are much to be preferred to thoſe of ſand †.

The ores of antimony and of lead frequently accompany thoſe of copper; and it has already been proved, that $\frac{1}{1920}$ of either of the former metals is ſufficient to deſtroy the ductility of gold. It may therefore be ſuſpected, that the brittle quality which certain kinds of copper communicate to gold, proceeds from thoſe metals; for, though other metallic ſubſtances produce the ſame effect, yet, as the former eſpecially are ſo commonly preſent with the ores of copper, it is highly probable that antimony, or lead, may remain combined with the ſmelted copper, in a proportion too ſmall to affect the general and more obvious properties of that metal, yet ſtill ſufficient to deſtroy the ductility of gold, when ſuch copper is employed as an alloy.

—moſt probably from antimony or lead.

To aſcertain how far copper might be alloyed with lead, or antimony, without any very apparent change in its obvious properties, the following experiments were made:

Copper may contain a notable portion of lead or antimony, and be very merchantable.

* Gold made ſtandard by platina, is not only very ductile, but alſo (when hammered) tolerably elaſtic. perhaps it might be advantageouſly employed for the ſprings of watches, &c. C. H.

† Bars of alloyed gold (particularly thoſe which are alloyed with copper) are generally diſcoloured on the ſurface, when caſt in moulds of ſand; but not ſo when caſt in iron. It may be ſuſpected, that the alloy is ſuperficially oxidized when ſand is employed, in conſequence of the air which is lodged in the interſtices, together, perhaps, with ſome degree of moiſture. C. H.

To 476 grains of fine malleable copper, in fuſion, four grains of antimony were added, and, being well mixed, the whole was poured into a mould.

The colour of this copper, when filed and poliſhed, was ſuch as not eaſily to be diſtinguiſhed from that which had not been thus alloyed.

It was alſo hammered and rolled, without ſhewing any ſigns of brittleneſs. The ſpecific gravity was 8,354 *.

The like quantity of copper was alloyed with four grains of lead.

This alſo was ductile, and did not ſuffer any apparent change of colour.

The ſpecific gravity was 8,472.

The ſame experiment was repeated with four grains of biſmuth; but the copper thus alloyed was exceedingly ſpongy and brittle.

It appears, therefore, that four grains of antimony, or of lead, may be preſent in one ounce, or 480 grains of copper, without producing any very apparent change in colour or ductility, and but little in ſpecific gravity; ſuch copper may, therefore, without ſuſpicion, be occaſionally employed to alloy gold; then, however, the antimony or lead will produce a powerful effect; for it has been proved, that $\frac{1}{1920}$ of either of theſe will deſtroy the ductility of gold. But, ſuppoſing one ounce troy of copper which contains four grains of antimony, or of lead, to be employed to alloy eleven ounces of gold, 24 carats fine, there would then be four grains of the above-mentioned metals in the 12 ounces or troy pound; and therefore the quantity of theſe would be conſiderably more than is required to deſtroy the ductility of gold. For the troy pound contains 5760 grains; and 4 is to 5760 as 1 to 1440; conſequently, this proportion much exceeds the quantity which is capable of producing the above-mentioned effect.

It often contains a very conſiderable proportion.

But the copper of commerce often contains a much greater proportion of one or other of theſe metals; and, although it then appears more pale than common, yet it has, without ſuſpicion, been purchaſed by thoſe who, from their profeſſion, are ſuppoſed to be competent judges, and who eſpecially

* The fineſt Swediſh copper was employed in theſe experiments; the ſpecific gravity of it was 8,895.

2 require

require copper to be as pure as poſſible. Perſons of this deſcription, however, are liable to be deceived; for, in 1791, Mr. Roitier, Director of the Mint at Paris, purchaſed a quantity of copper from the mines of Poullaoen in Britany; but he ſoon diſcovered, from the effects which it produced, when employed as an alloy, that it was not pure, and therefore requeſted Mr. Sage to examine it. By the latter, it was analyſed, and was found to contain one forty-eighth of antimony *.

Antimony is moſt frequently the noxious addition.

Allowing, therefore, that other metallic ſubſtances may at times be preſent in copper, and may contribute to affect gold which is alloyed with it, yet, for the reaſons above related, the author is inclined to attribute, moſt frequently, this effect to antimony or lead.

Copper which is pure, is uniform in its effects, and does not injure the ductility of gold; it would therefore be proper in all caſes when copper is to be purchaſed for the purpoſe of alloying gold, to make a previous trial of it on a ſmall quantity, as this would anſwer every purpoſe of a tedious and expenſive analyſis.

Farther examination of copper in the market.

Since the above was written, he made various experiments in the humid way, on the different kinds of copper, which are known in commerce, eſpecially on the following:

No. 1.	Fineſt granulated Swediſh copper	ſp. grav.	8,895.
2.	—— Swediſh dollar copper	ſp. grav.	8,799.
3.	—— ſheet Britiſh copper	ſp. grav.	8,785.
4.	Fine granulated Britiſh copper	ſp. grav.	8,607.

480 grains of the firſt, only afforded a few particles of ſulphate of lead, which could not be eſtimated.

The ſecond contained both lead and antimony, of which the lead was in the largeſt proportion, as it amounted to nearly one grain of metallic lead, whilſt the antimony did not exceed half a grain.

The ſheet Britiſh copper yielded ſome lead, with ſcarcely any antimony; and, on the contrary, the granulated Britiſh copper contained antimony with but very little lead. We may therefore conclude, that the varieties of copper known in commerce, are ſeldom, if ever, abſolutely free from lead or antimony; and that the brittle quality, ſo frequently communicated to gold by an alloy of copper, ariſes from the preſence of one

* *Journal de Phyſique*, 1792, Tome XL. p. 273.

or

or both of these metals, which, even in the proportion of $\frac{1}{1920}$ part of the mass, was already proved to be capable of destroying the ductility of gold.

Facts respecting Swedish copper.

Mr. H. lately made some farther inquiries concerning the varieties of Swedish copper, and was informed, that the fine granulated copper is made in this country from the Swedish cake-copper, merely by the ordinary process of ganulation; and, as the quality even of this copper has been found variable, the Deputy Master of the Mint has of late employed British copper, which has been refined expressly for the purpose, and seems to answer perfectly well. Respecting the variable and occasional very bad quality of the copper dollars, Mr. Swedenstierna, a learned Swedish gentleman at present in London, has favoured him with some particulars, in a letter, of which the following is an extract *.

Copper dollars.

"But with regard to dollars, I should be much surprised if they had ever been perfectly pure; because, as far as I know, they have always been made of the copper of Fahlun, of which the ores have always been more or less mixed with sulphurated lead, and perhaps antimony. However, as these dollars were originally struck under the Reign of *Frederic* king of Sweden, and it is possible that the ores at Fahlun might have been then purer, it is probable that these dollars may have been purer at first than those which have since been counterfeited; for these dollars having been called for in India, and ordered more particularly in considerable quantity, by the Asiatic Company of Copenhagen, others have been struck since repeatedly, and of all kinds of copper without distinction. This fact, at least, explains the cause of their inequality of composition."

"It remains to be shewn whether any of these dollars are perfectly pure. For my part, I am most inclined to think, that the demand in India has arisen from the convenience of form, and that prejudice has given them credit and circulation as money. For several years of late, the Danes, having found this copper too dear, have themselves counterfeited the mark, and struck them at Rocraas in Norway; and this must afford another variety of this copper."

* The Extract is in French, of which I give the translation. *W. N.*

GOLD

GOLD ALLOYED WITH SILVER.

Gold alloyed with pure ſilver, in ſtandard proportion, is ſo generally known, that it would be needleſs here to ſay more, than that it approaches the neareſt to the ductility of fine gold, and that the ſpecific gravity of this mixture differs but very little from that which, according to calculation, would reſult from the relative proportions of the two metals. Gold and ſilver.

From the foregoing experiments it is evident, that many of the metallic ſubſtances with which gold may be alloyed, are more or leſs liable to be ſeparated from it during fuſion, in conſequence of their relative affinities with caloric, with oxygen, or with both; and that theſe affinities become modified, by thoſe which prevail between the various metallic ſubſtances and gold. Moreover, it is evident, that even the moſt oxidable metals have this property much diminiſhed or checked by being united with gold, which appears ſo to envelope and retain their particles, as to impede the uſual influence of heat, as well as the natural exertion of their affinities with the oxigen of the atmoſphere. The following experiment was therefore made, to aſcertain the comparative loſs cauſed by the volatilization, or by the oxidizement of various metallic ſubſtances, when added to gold during a given period of fuſion, and under ſimilar circumſtances. General remarks on alloys of gold.

Experiment.

Ten four-inch crucibles, which had been previouſly made red-hot, were put into as many twelve-inch crucibles, which were placed in wind furnaces of ſimilar conſtruction, and heated as equally as poſſible. Each of the ſmall crucibles contained five ounces ten pennyweights and fourteen grains of gold, 23 car. 3½ grs. fine, which being completely melted, nine pennyweights and ten grains of the ſeveral metals were added, and mixed in the uſual manner, after which, the fuſion was continued in the open veſſels during one hour Experiment on the loſs by heating the different alloys.

The different maſſes, when cold, were weighed; but, previous to this, the ſcoria or glaſs which had been formed on ſome of them was gently removed.

It appears, that fine gold, gold alloyed with ſilver, gold alloyed with copper, and gold alloyed with tin, did not ſuffer any loſs during the experiment. Reſults of loſs by ſtrong heat.

More-

Moreover, that gold alloyed with lead only loſt three grains, chiefly by vitrification.

That gold alloyed with iron loſt 12 grains, which formed ſcoria.

That gold alloyed with biſmuth alſo loſt 12 grains chiefly by vitrification.

That gold alloyed with antimony loſt the ſame quantity, partly by volatilization, and partly by vitrification.

That gold alloyed with zinc loſt one pennyweight, by volatilization. And,

That gold alloyed with arſenic, not only loſt the whole quantity of alloy, but alſo two grains of the gold, which were carried off in conſequence of the rapid volatilization of the arſenic.

Lewis, (Phil. Comm. of Arts, p. 88,) however, aſſerts that "gold is more volatilized by antimony than by arſenic or zinc; "but to produce this effect the fire muſt be vehement, the "crucible ſhallow, and the air ſtrongly impelled." Theſe circumſtances, according to their variations, muſt undoubtedly very much influence the reſults of ſuch experiments; and therefore, although the reverſe was found to take place in the experiments here ſtated, it does not follow that certain changes ſhould not be produced by different degrees of heat, by the figure of the veſſels, and by a current of air more or leſs ſtrong.

Silver and copper only are fit for the alloy of gold.

The whole of the experiments of this ſection tend to prove, that (agreeably to general practice and opinion) only two of the metals are proper for the alloy of gold coin, namely, ſilver and copper; as all the others either conſiderably alter the colour, or diminiſh the ductility of gold. In reſpect to the latter quality, the different metallic ſubſtances which have been employed in the preſent experiments, appear to affect gold nearly in the following decreaſing order.

Order of effect as to ductility.

1. Biſmuth. 2. Lead. 3. Antimony. (Theſe are nearly equal in effect.) 4. Arſenic. 5. Zinc. 6. Cobalt. 7. Manganeſe. 8. Nickel. 9. Tin. 10. Iron. 11. Platina *. 12. Copper. 13. Silver.

* Had the platina been quite pure, the compound metal would probably have poſſeſſed more ductility; I cannot therefore take upon me to aſſert poſitively, that the place here aſſigned to platina, is preciſely that which it ought to occupy.

SECTION

SECTION III.*

ON THE COMPARATIVE WEAR OF GOLD, WHEN ALLOYED BY VARIOUS METALS.

Nature of the friction to which coin is expofed

Gold, when in the form of coin, appears to be generally expofed to three varieties of friction, *viz.*

1ft. Friction between pieces of gold coin of a fimilar or of a different quality.

2d. Friction of gold coin againft coin of other metals, fuch as filver and copper.

3d. The friction which gold coins of various qualities fuffer, when expofed to the action of certain fubftances, fuch as the particles or filings of metals, gritty powders, &c.

The confideration of thefe different modes of wear, points out the beft method to be purfued in an experimental inveftigation.

The whole of the experiments which compofe this fection may therefore be divided into three fubordinate feries; the two firft of which have been directed to the confideration of that part of the diminution of the coin which arifes from the rubbing of one piece of metal againft another; while,

The third of thefe fubordinate feries was intended to fhow the comparative power of gold, differently alloyed, to refift abrafion from fand or other gritty powders.

Defcription of the experiments, coin againft coin, fingly.

In the firft fet of experiments, 28 pieces of coin were fixed to a frame, and over each of them was placed another piece of coin, which was preffed againft it by a weight. Thefe upper pieces were all attached to a fecond frame, fo that, by means of the motion communicated thereto by cranks, each upper piece was made to move about $\frac{3}{8}$ of an inch backwards and forwards on the lower one. This mode of experiment afforded an opportunity of trying the comparative diminution of gold differently alloyed, both when rubbed againft pieces of the fame and of a different alloy; and alfo of examining the difference of wear between pieces with plain and with ftamped faces.

* The facts detailed in this important memoir having extended my abridgement beyond my firft eftimate, I find myfelf under the neceffity either of tranfpofing the 2d and 3d fections *on the Specific Gravity and the comparative Wear of Gold,* or elfe of feparating Plate XIV. from its defcription.

In

many pieces in a box.

In the fecond feries, 200 pieces of gold, differently alloyed, were inclofed in a wooden box, of a cubic figure, which was kept conftantly turning round, till, by the repeated rubbing and ftriking of the pieces againft each other, and againft the fides of the box, they were found to be fenfibly diminifhed. This, like the experiments of the firft fet, was intended to fhow the comparative diminution of gold differently alloyed; but, whereas that fhewed the effect of rubbing only, this fhewed the joint effect of rubbing and ftriking, and was intended to imitate (although in a more violent degree) the effect produced upon coin by pouring it out of one bag or drawer into another.

Coin rubbed on a flat furface.

The experiments of the third fet were made by preffing the pieces to be examined againft the rim of a flat horizontal wheel, by means of equal weights, fo that, by turning the wheel round, they all fuffered an equal degree of friction. That part of the wheel againft which the pieces rubbed, was fprinkled or coated with fome kind of powder, which was occafionally varied.

The above ftatement will convey a general idea of the manner of making the experiments; but, that the whole may be more fully comprehended, the following defcription of the inftruments has been added by Mr. Cavendifh.*

DESCRIPTION OF THE INSTRUMENTS.

Defcription of machinery for determining the effects of friction on coin.

In the firft feries of experiments, 28 pieces of coin were fixed to a frame, and over each of them was placed another piece, which was preffed againft it by a weight; and thefe upper pieces were all connected to a fecond frame, fo that, in confequence of the motion communicated thereto by cranks, each upper piece was rubbed backwards and forwards upon that which was under it.

Fig. 1, (Plate XIV.) reprefents a plan of this inftrument; and Fig. 2 is a vertical fection of it, drawn parallel to the line AB.

The upper frame, or that to which the upper pieces of coin are connected, is of brafs, and confifts of four bars, Fig. 1, AB, B*b*, *ba*, and *a*A, with three crofs bars C*c*, C*c*, C*c*.

* The inftruments were made by Mr. Cuthbertfon, of Poland-ftreet, who alfo had the care of them during the experiments which were made at his houfe.

The

The lower frame confifts of a board, placed immediately under the upper frame, and is expreffed in Fig. 2, by the letters LL.

Defcription of machinery for determining the effects of friction on coin.

The upper frame is fupported by two vertical boards, extending the whole length of the fides B*b* and *Aa*, fo that the ends of them are feen in Fig. 2, and are denoted by the letters DD, DD. Thefe boards are faftened to the upper frame, and to the table upon which the apparatus ftands, by hinges, fo that the upper frame can move freely in the direction BA, but can have no motion in the direction perpendicular thereto. Thefe vertical boards are omitted in Fig. 1; for, as the intention of this defcription is not to give a detail of all the parts of the inftruments, but only to explain their manner of acting, I have taken the liberty to omit fuch parts as tended to produce an intricacy in the figures, without being neceffary to this object.

The difpofition of the pieces of coin on the frames, is reprefented in Fig. 1. N*nn* denote one of the connecting pieces, by which the upper pieces of coin are connected to the upper frame, and in which the fmall circle reprefents the pofition of the coin; the large circle is the part which fupports the weight, and *nn* the part by which it is connected to the upper frame.

To avoid confufion, neither thefe connecting pieces nor the pieces of coin are reprefented in Fig. 2; but, inftead thereof, a fection of one of thefe pieces is given in Fig. 3, upon a larger fcale.

In this figure, LL is the lower frame, and C one of the bars of the upper frame; *y* is one of the lower pieces of coin, which is bedded and fixed firmly in a brafs focket *x*, faftened to the lower frame; *u* is the piece of coin to be rubbed againft it, which, in like manner, is fixed in another brafs focket *w*; N*n* is the connecting piece, by which this focket is connected t the bar C of the upper frame. This piece turns on pivots, in two ftuds *n*, fixed to the bar C, fo that it can turn freely on thofe pivots in a vertical direction, but cannot be perceptibly fhaken horizontally.

Z is the weight by which this connecting piece is preffed down; it is round, and is placed with its centre exactly over that of the focket *w*.

It muft be obferved that, in the conftruction of this machine, three things principally demanded attention.

1ft. That

Defcription of machinery for determining the effects of friction on coin.

1ft. That the pieces of coin fhould all move equally.

2dly. That they fhould all be preffed againft the lower pieces by the fame weight. And,

3dly. That they fhould bear flat againft them.

As to the firft requifite, it is evident that the pieces muft all move alike, excepting fo far as proceeded from the fpringing of the parts of the machine, or from the fhake in its joints, both of which were very fmall.

Secondly, as the connecting pieces move freely in a vertical direction, it is clear that the force with which the upper piece of coin is preffed againft the lower one, depends only on its own weight, on that of the focket w, on that of the connecting piece Nn, and on the weight Z by which it is loaded ; fo that the fecond requifite is thus eafily obtained.

Thirdly, The connecting piece Nn bears againft the focket w only by the pin p, which enters into a hole in the centre of the focket, fo that the two pieces muft neceffarily bear flat againft each other; but, as this pin alone would not have prevented the focket from turning round on its centre, two other pins Π Π were fixed into the connecting piece, and entered into flits made in the focket near its circumference, allowing no more fhake than was neceffary to prevent it from fticking; and thus the motion round the centre was effectually prevented.

It may be obferved, that the pieces might have been made to bear flat againft each other by fixing the fockets w in gimbals; but, as the method above defcribed was effectual, and much eafier made, it was preferred.

It may be alfo remarked, that the breadth of the bars Cc, as reprefented in Fig. 1, is not fufficient to prevent them from fpringing confiderably; for this reafon, a method of ftrengthening them was employed, which anfwered the purpofe perfectly well, but is omitted in the drawing, as it could not be eafily reprefented.

It was at firft intended, that the lower frame fhould have remained fixed, and that only the upper one fhould have moved; but, in a previous trial, in which two pieces of metal were rubbed backwards and forwards upon each other in the fame line, with a view to difcover what weight would be neceffary to make the pieces wear tolerably faft, it was found that for a time they diminifhed flowly, but that little furrows or gullies were foon worn in them, and that then the diminution was rapid

rapid. It was alfo obferved, that the gullies in the upper pieces correfponded to thofe in the lower ones; fo that it was impoffible that the pieces of metal fhould touch each other in thofe places where the diminution was moft rapid, and confequently the gullies muft have been formed by the particles of metal which had been abraded, and which fubfequently had become accumulated.

Defcription of machinery for determining the effects of friction on coin.

It feemed to Mr. C. that the moft probable way to prevent the little furrows or gullies from being thus formed, would be, to conftruct the inftrument in fuch a manner, that the direction in which the pieces rubbed upon each other fhould continually vary. The following contrivance was therefore adopted, by which the pieces were prevented from rubbing together twice in the fame direction.

In this method, the lower frame, as well as the upper, is fupported on two moveable vertical boards; but, whereas the boards fupporting the upper frame are placed parallel to B*b*; in confequence of which the frame can move only in the direction BA, thefe are placed parallel to BA, fo that the frame can move only in the direction B*b*.

EE is the axis by which the upper frame is moved: this turns in fixed fockets at SS, and is turned at each end into the form of an eccentric circle, which acts as a crank; fo that, by means of the levers EK, which at one end turn on thefe eccentric circles, and at the other end turn on joints fixed to the upper frame, this frame is made to move ¼ of an inch, in the direction BA, during one half of the revolution of the axis, and as much in the contrary direction, during the other half revolution.

ee is an axis of the fame kind, ferving to move the lower frame. HH is a windlafs, which turns thefe two axes by means of the toothed wheels F, *f*, which work in the toothed wheels G, *g*, faftened to the axes EE and *ee*. TTTTT is the table upon which the apparatus ftands.

The wheel F has 90 teeth, *f* has 75, and G, *g*, have each 20; fo that the axis EE makes fix revolutions while *ee* makes five; and, at a medium, thefe axes make about four revolutions to one of the windlafs. A counter is placed fo as to fhow the number of revolutions of the windlafs.

If the two frames had performed their vibrations in the fame time, no advantage would have been gained, for the pieces of coin

Defcription of machinery for determining the effects of friction on coin.

coin would ftill have moved upon each other always in the fame line; but, as their vibrations are performed in different times, Mr. C. fhews, that the effect muft be quite different.

This contrivance, therefore, effectually prevented the pieces from moving upon each other always in the fame line; and it feems alfo to have much diminifhed the difpofition which they had to wear in gullies, but not intirely; for, from the following experiments it appears, that ftill fome few particles would become occafionally collected, and then acted as a grinding powder, which accelerated the wear of the pieces. This was obferved particularly to happen to the pieces of gold alloyed with an equal proportion of copper, and to the pieces of copper, which were alfo more frequently worn in furrows or gullies, than the other pieces of more ductile metal.

The motion of the pieces of coin upon each other, is greater than it would have been if only one frame had been made to move, nearly in the proportion of 3 to 2; fo that the whole motion of the pieces, in each femi-revolution of the axes EE or *ee*, is about $\frac{3}{8}$ of an inch, and therefore it is about three inches in each revolution of the windlafs.

The inftrument employed in the fecond feries of experiments, is fo fimple as not to require any drawing. It confifted only of a cubical box of oak, which meafured eight inches each way, within fide. This box was moved by the axis EE of the former inftrument, which was paffed through the middle of two oppofite fides, and was fixed in that pofition.

Fig. 4, reprefents a plan of the inftrument ufed in the third feries of experiments. *aaa* is a horizontal table, turning upon a vertical axis; and BBB*bbb* is a fixed frame furrounding it.

The pieces of coin are faftened to this fixed frame, by the fame connecting pieces which were formerly employed, and are preffed down alfo by fimilar weights. The diameter of that part of the wheel againft which the centres of the pieces of coin are preffed, is 29 inches; fo that, while this wheel makes one revolution, the pieces are rubbed againft it through the whole circumference of this circle, that is, through $91\frac{1}{10}$ inches

A fhallow groove *ggg* is cut in this wheel, in that part againft which the pieces are preffed, in order to confine the powders employed in the experiments; and the number of revolutions of the wheel are marked by a counter.

By

By the help of the inſtruments above deſcribed, it was propoſed to determine, as accurately as poſſible, Deſcription of machinery for determining the effects of friction on coin.

1ſt. The comparative wear of ſoft and of hard gold.

2dly. Whether coins with flat or with raiſed ſurfaces ſuffer the greateſt loſs by friction, when ſubjected to it under ſimilar circumſtances*.

It is ſcarcely neceſſary to obſerve, that rigorouſly exact reſults could not be expected in all the minutiæ of experiments like the preſent; for, many circumſtances, apparently but trivial, produced almoſt unſurmountable obſtacles; but, nevertheleſs, theſe did not impede the eſſential objects from being inveſtigated, and determined, in a manner ſufficiently ſatisfactory.

Before the experiments are deſcribed, it will be proper to add, that, to obviate the irregular effects which would be produced by the inequality of the impreſſions uſually employed for coins, Mr. Cavendiſh ſuggeſted a die, which was executed by Mr. Pingo, and which conſiſted of round prominencies regularly diſpoſed over the ſurface, ſo that the effects which this impreſſion produced, during friction, were uniformly the ſame in every direction.

The firſt experiments were intended to aſcertain the different wear of gold made ſtandard by various metals; and the pieces were rubbed againſt each other by meaus of the firſt-deſcribed apparatus, which the author calls No. 1.

Some preparatory experiments were alſo made, to try the effects of this machine, as well as to determine, in ſome meaſure, the comparative wear of gold made ſtandard by copper, of a mixture of gold and copper in equal proportions, and, laſtly, of copper.

* Although coins with protuberances on their ſurfaces, have been generally ſuppoſed to ſuffer more by friction than thoſe which are flat, yet, as this opinion has been queſtioned, and as ſeveral objections have been made to it by intelligent perſons, it was thought expedient that the deciſion of the queſtion ſhould form part of the preſent inveſtigation.

(To be continued.)

XIII.

Obſervations of the Tranſit of Mercury over the Diſk of the Sun; to which is added, an Inveſtigation of the Cauſes which often prevent the proper Action of Mirrors. By WILLIAM HERSCHEL, *LL. D. F. R. S.* *

Tranſit of Mercury.

THE following obſervations were made with a view to attend particularly to every phenomenon that might occur during the paſſage of the planet Mercury over the ſun's body. My ſolar apparatus, on account of the numerous obſervations I have lately been in the habit of making, was in great order for viewing the ſun in the higheſt perfection; and, very fortunately, the weather proved to be as favourable as I could poſſibly have wiſhed it.

The time at which the obſervations were made, not being an object of my inveſtigation, is only to be conſidered as denoting the order of their ſucceſſion.

Mercury ſeen on the ſun's diſk, perfectly round;

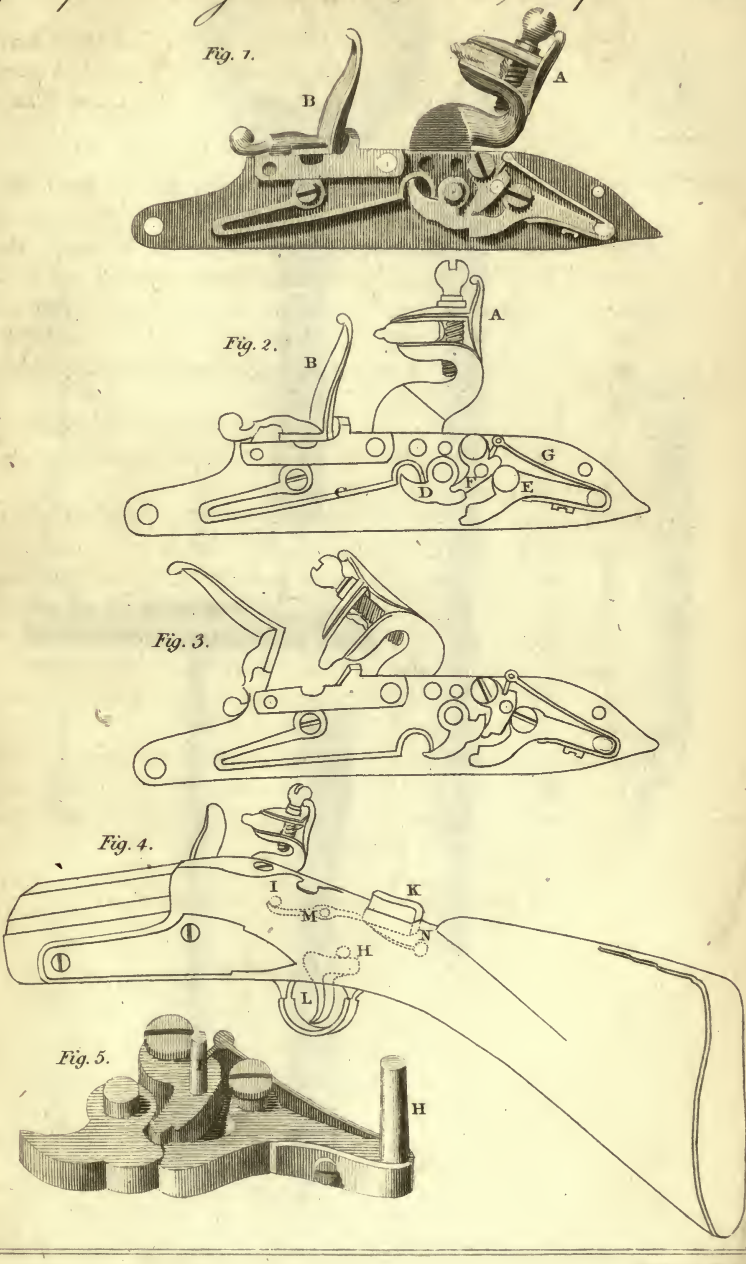
November 9, 1802. About 40′ after ſeven o'clock in the morning, I directed a teleſcope, with a glaſs mirror of ſeven feet focal length, and 6,3 inches in diameter, to the ſun; and perceived the planet Mercury. It was eaſily to be diſtinguiſhed from the openings in the luminous clouds, generally called ſpots, of which there were more than forty in number. Its perfect roundneſs would have been ſufficient to point it out, had I not already known where to look for it.

$10^h\ 0'$. When the ſun was come to a ſufficient altitude to ſhow objects on its ſurface with diſtinctneſs, I directed my attention to the contour of the merculial diſk, and found its termination perfectly ſharp.

* From the Philoſophical Tranſactions for 1803.

(To be continued.)

Mr. John Webb's Gun Lock for preventing Accidents in the use of Fire Arms



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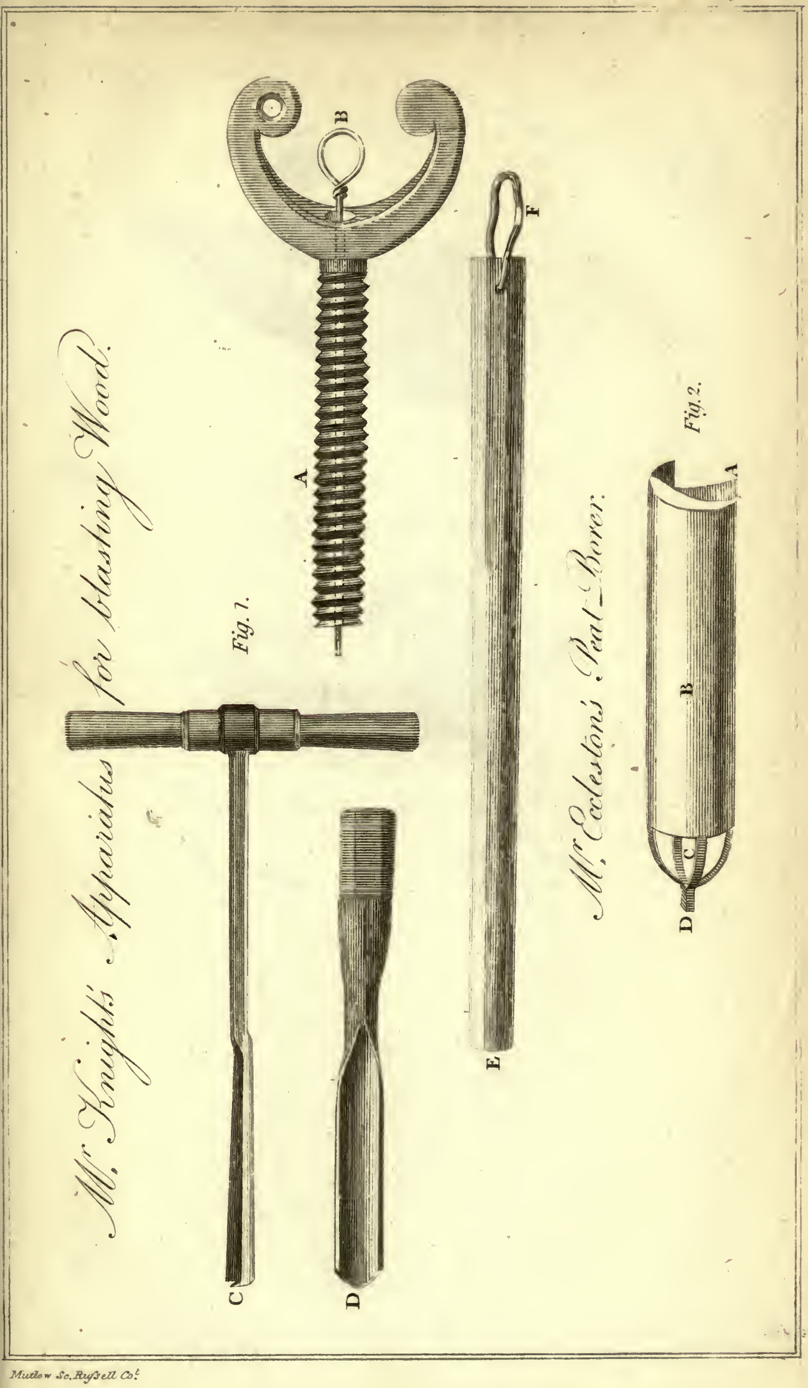
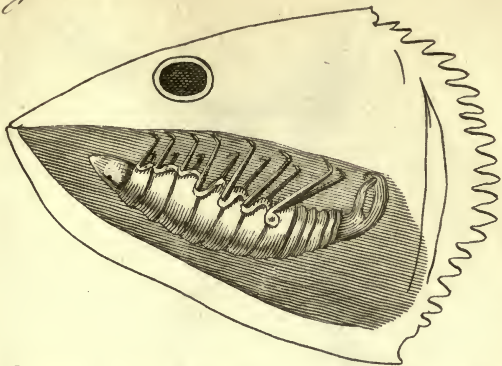
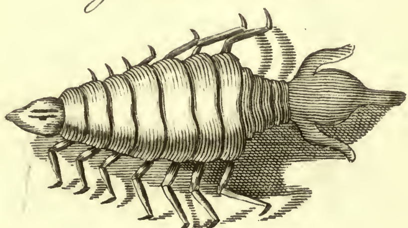


Mutlow Sc. Russell Co^t.

The Insect, as it places itself in the mouth of the Clupea tyrannus.



The Oniscus praegustator, drawn to its natural size, by measurement.



Leaches found upon the Insect.

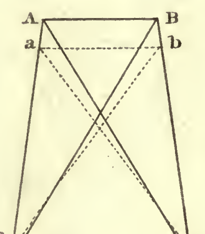


Fig. 3.

Mutlow, Sc Russell Ct.

Philos Journal Vol. V. Pl. IV. facing p. 64.

Outline of the Clupea tyrannus, correctly drawn to two thirds of its natural size.

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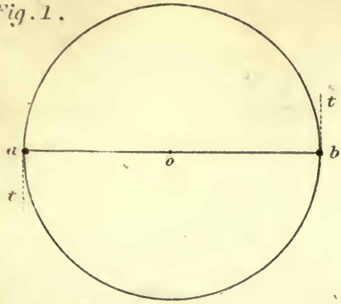
Dr Herschel on the Construction of the Heavens

Fig. 1.



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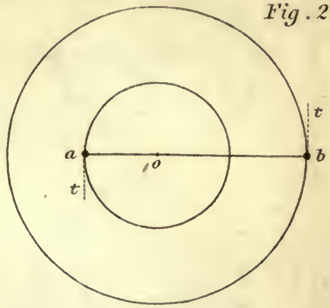

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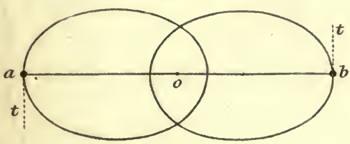

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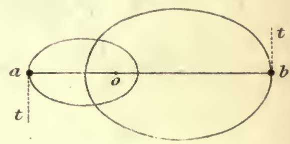

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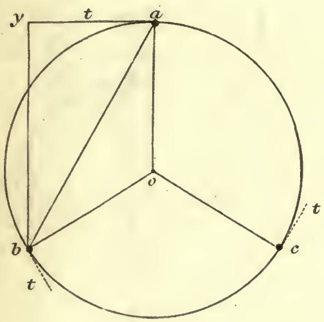

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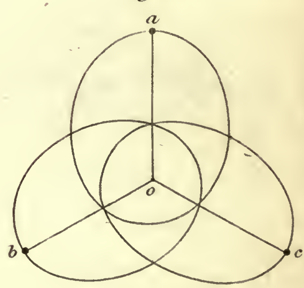

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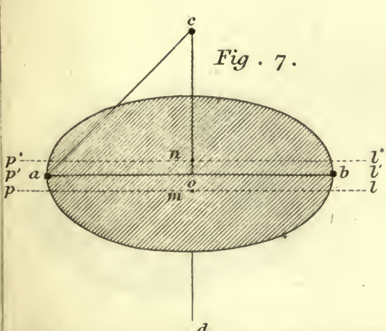

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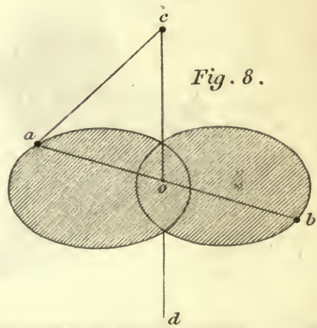

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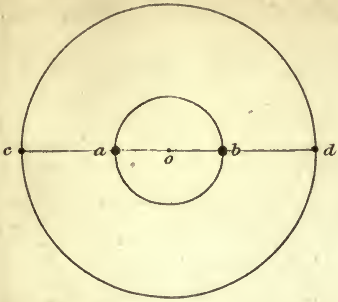
Fig. 9.



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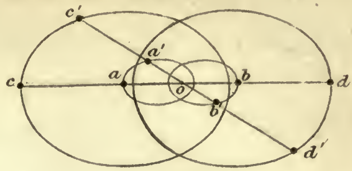

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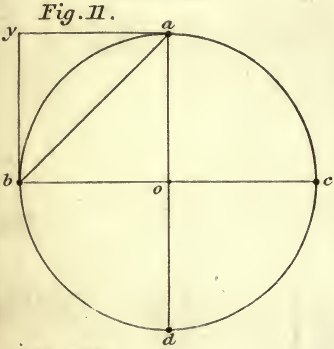

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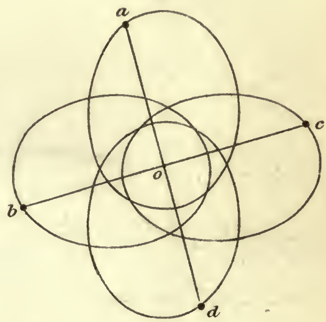

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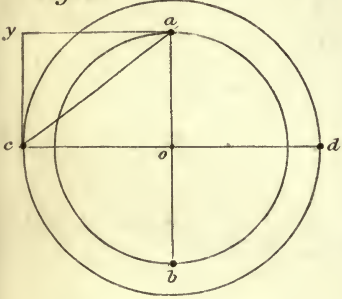

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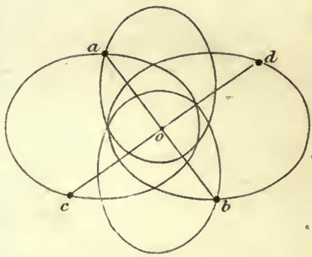

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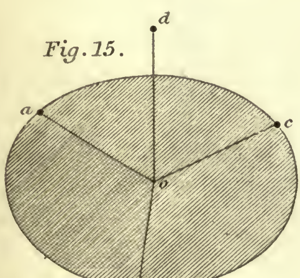

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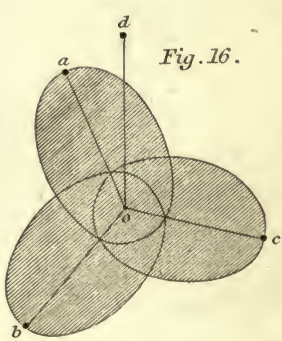

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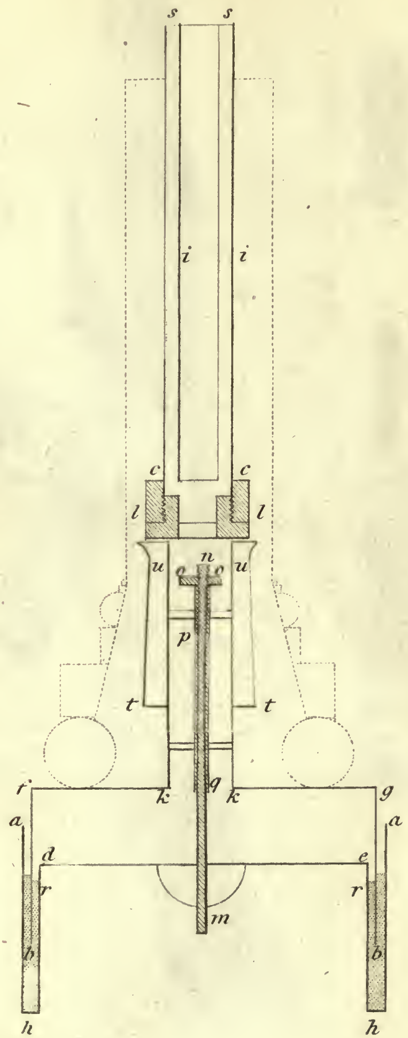
New Statical Lamp, by Sir A. N. Edelcrantz.



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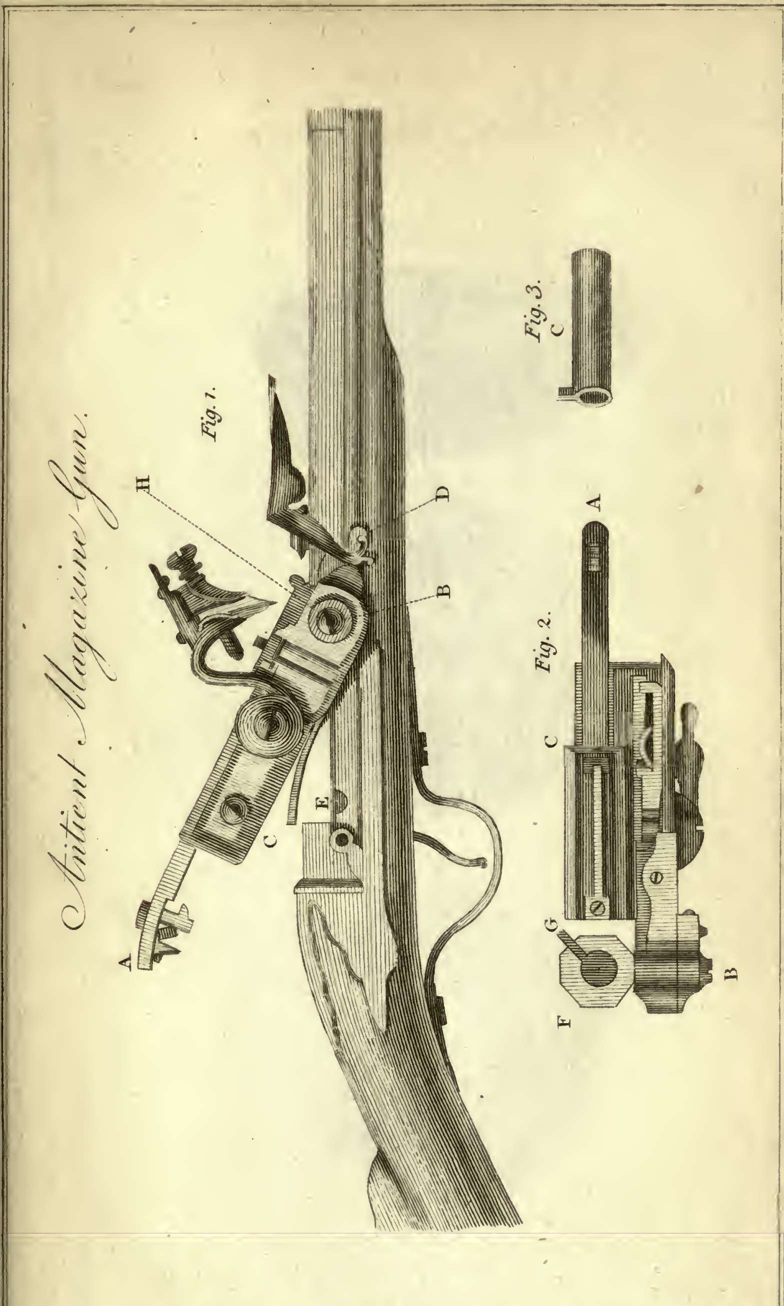
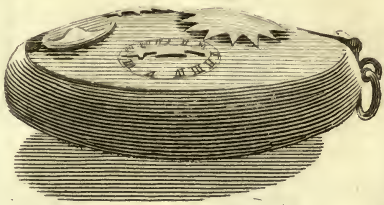


Antient Magazine Gun.

Mutlow Sc. Russell Cou

Ancient Lock.

Fig. 1.



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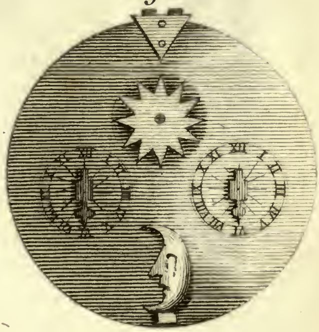

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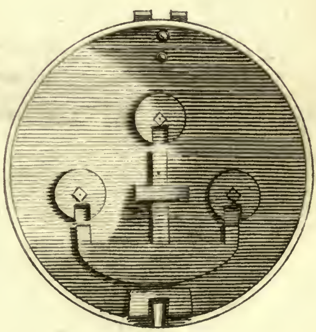

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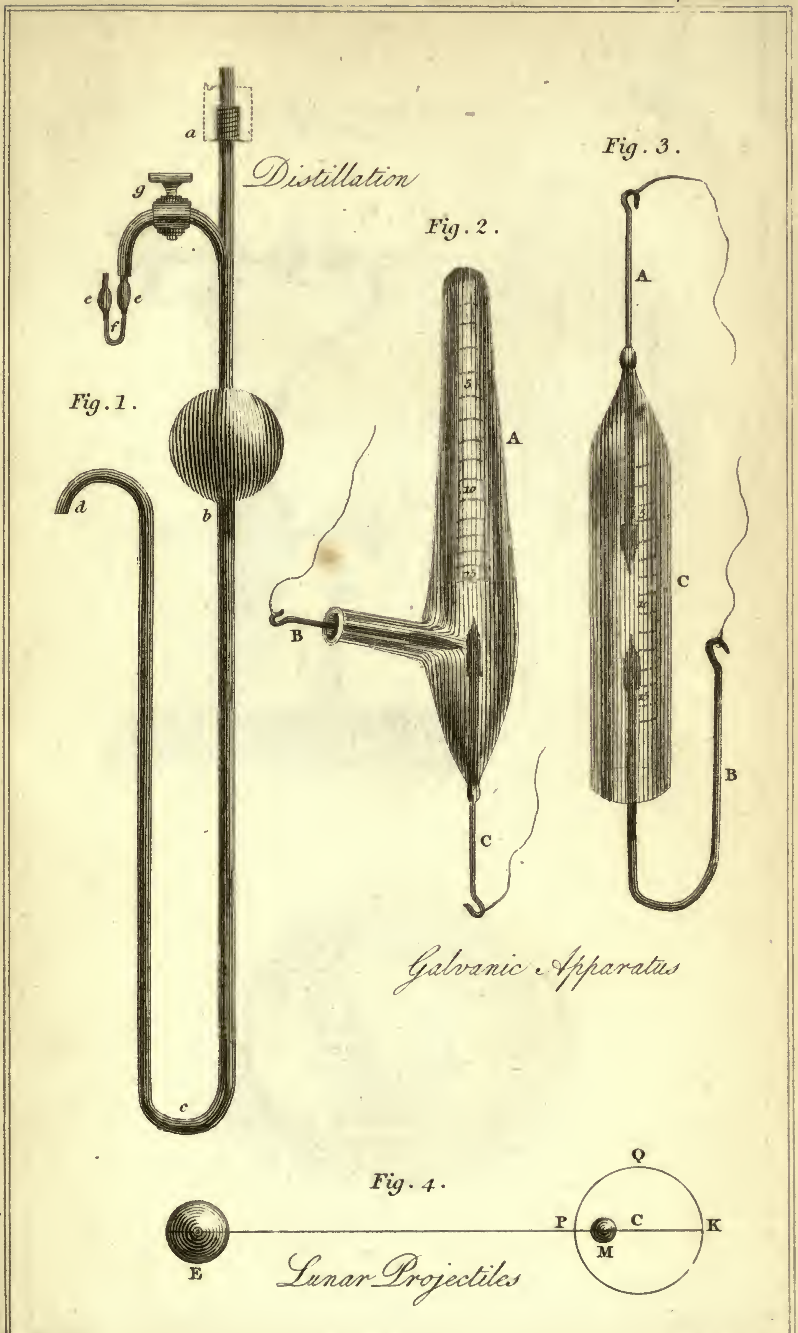

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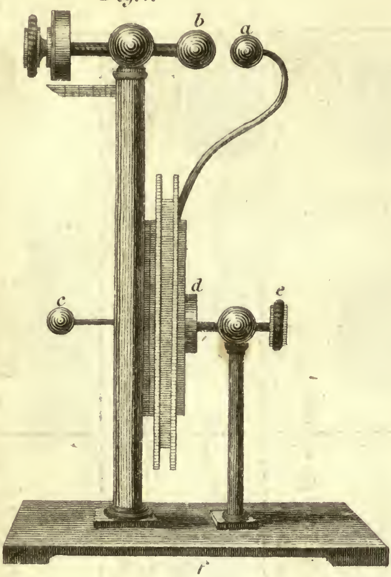
Talc Battery

Fig. 1.



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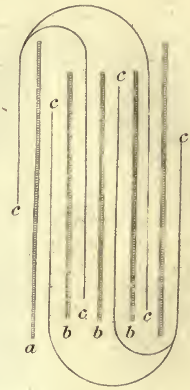
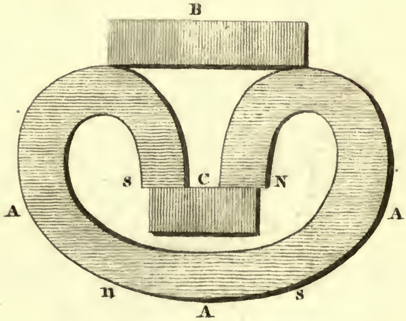


Fig 3



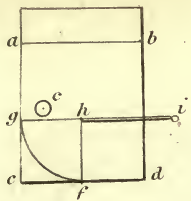
Artificial Magnet

Blunt delin.^t Mutlow Sc. Russell Co.^t

Telegraph used in Sweden

Invented by Sir A. N. Edelcrantz.

Fig 2



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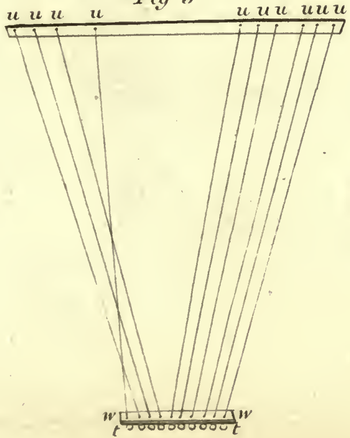

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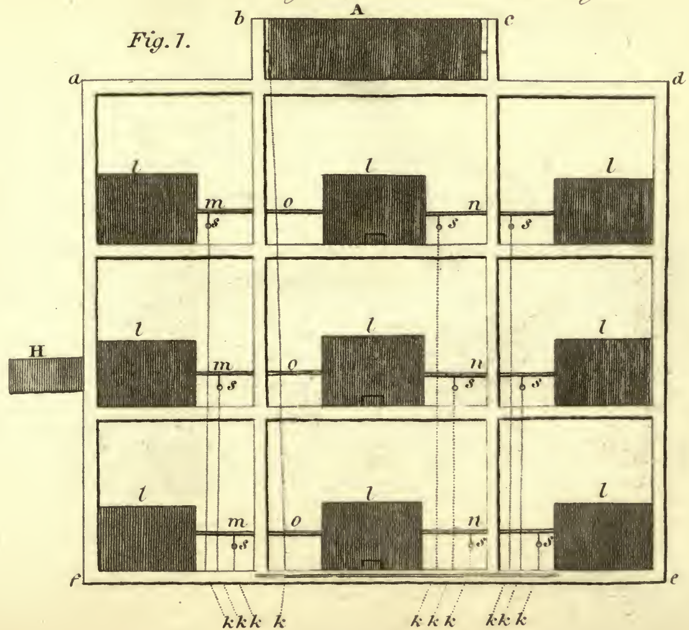

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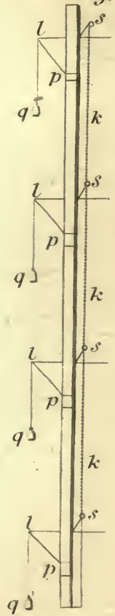
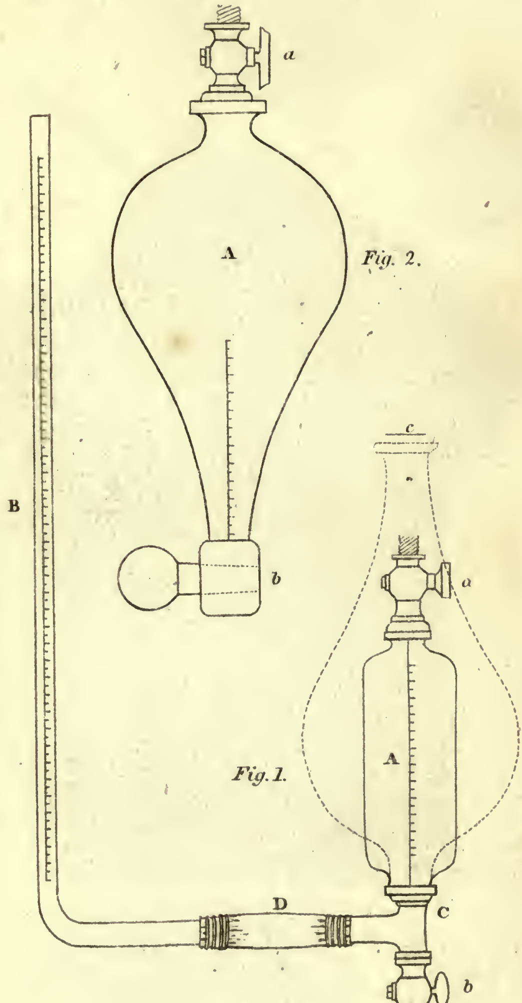


Mutlow Sc. Russell

Mr Henry's Apparatus for measuring the Absorption of Gases.



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Apparatus for determining the comparative wear of Gold, by the Hon.ble H. Cavendish Esq.r

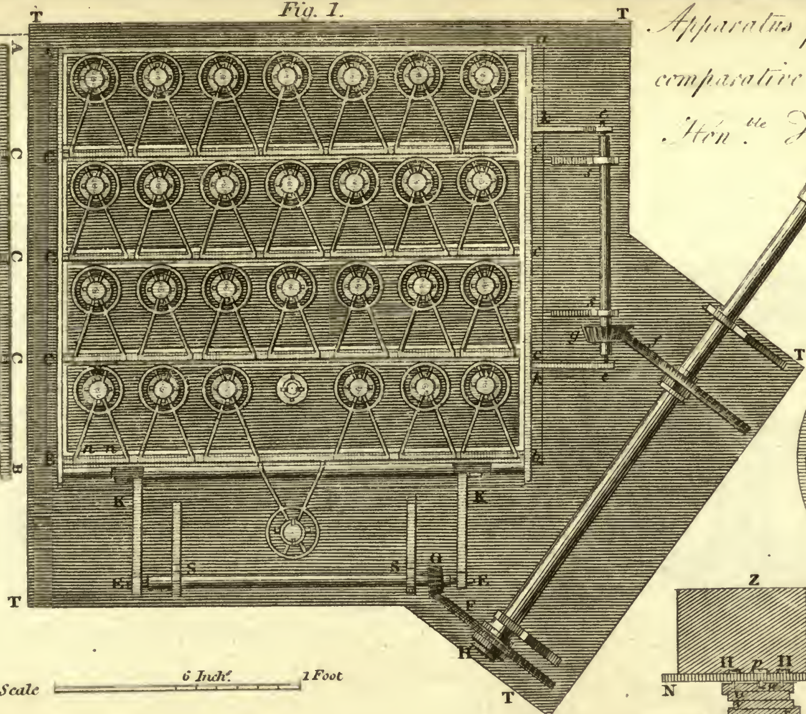
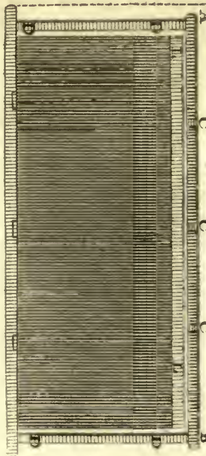


Fig. 1.



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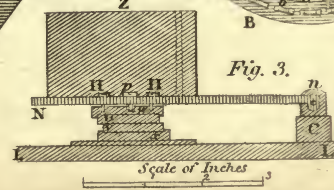

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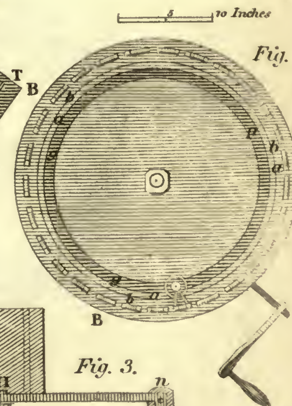


Fig. 4.

Subterraneous Graphometer,
by M. Komarzewski.

Fig. 2.

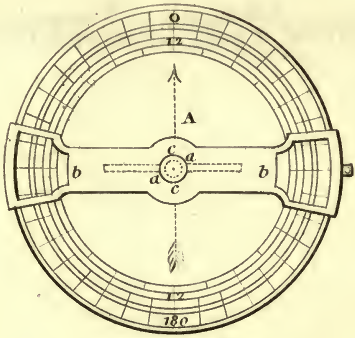
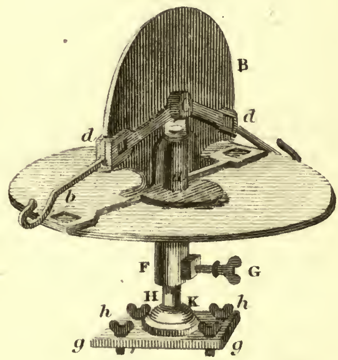


Fig. 1.



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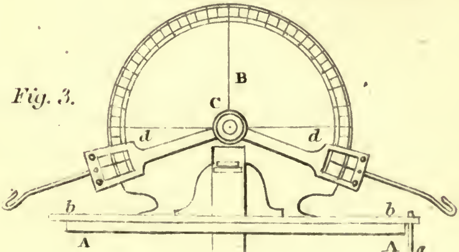

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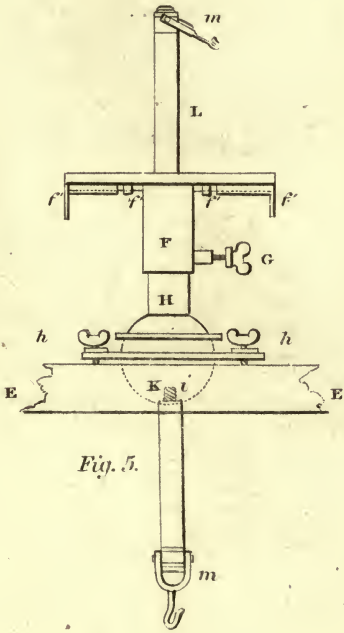

Fig. 5.

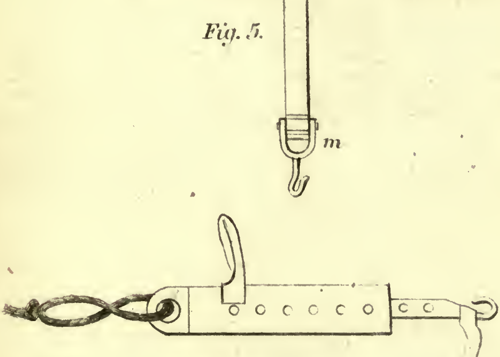

Fig. 6.

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Mutlow Sc. Rufsell Ct.

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